AMERICAN VETERINARY REVIEW,

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EDITORIAL.

AMERICAN VETERINARY COLLEGE—its organization under the general law of the State of New York—clamors against the legality of the institution, and objections to its power to issue diplomas—an opinion of the Attorney General of the State applied for and given, which will probably tend to secure a degree of comparative future tranquility—the trial and decision in the case of the United States Medical College excites its jealous enemies—they dare not test its legal right to existence, but persevere in their attacks—the Alumni Association decide to test the case before a court of law—the trustees of the college, not satisfied with this, apply to the Legislature and a special act is passed reaffirming the past action of the college and recognizing its legal existence, with increased privileges. HOG CHOLERA—if not rouget, what is it—the theory of prophylaxy by vaccination—we had been led to recognize two diseases under one name—the failures in Nebraska and in the Bureau of Animal Industry—it is not the same disease, inasmuch as the vaccine of one is not available for the other—experiments with the vaccine of rouget may introduce a serious disease in the United States—though not rouget, is it schweineseuche—it should be investigated, but a vaccine may be looked for. GANGLIONAR LEUCOCYTHEMIA—an unusual case, the entire cutaneous lymphatic structure being affected—diagnosis established by the numeration of the white corpuscles—the first case recorded in the United States. NUMBER OF DEAD HORSES IN NEW YORK CITY—useful statistics from Dr. S. Field—importance of the subject. REPORTS OF VETERINARY SOCIETIES—their transactions more interesting and more valuable to American veterinarians than to any others—the REVIEW has always been ready to give their reports to the public—takes pleasure in receiving them—ought to be recognized as their accepted organ. THE "REVIEW" AND UNITED STATES VETERINARY MEDICAL ASSOCIATION PRIZES—the first paper received—it treats of an important subject—the attention of the Committee on Prizes called to it—more candidates expected.

THE AMERICAN VETERINARY COLLEGE.—When, in 1875, the first announcement of the Faculty of the American Veterinary College was issued, a howl was at once started against that institution and its alleged illegal existence, though it had been

carefully organized and incorporated under the General Law of the State, as other large and important institutions in the State had been, and which had no other legal right to work. This fact, however, its enemies refused to consider. What was a good and sufficient authorization for other enterprizes, failed, in their estimation, when applied to the case of the American Veterinary College, and the sudden collapse of an institution which was operating under false pretences (?) was confidently and complacently predicted. But the why and wherefore was a question no one could satisfactorily answer, for there were none but imaginary reasons for such a forecast; "the wish was father to the thought."

The establishment of the A. V. C. was a good work and originated in a commendable purpose, and those connected with it were not to be alarmed by the howling of a few discontented and jealous spirits in opposition. Still, there lingered a degree of doubt in the minds of some of its timid friends, who seemed to be impressed with an apprehension of a lack of some sort of substantiation which ought to be available, though there was the law, positive and tangible, to which reference was easy, and which every man could read for himself. But for all this, some vaguely conceived and undefined desirable element seemed wanting, and some sort of positive self-assertion appeared to be demanded.

Under these circumstances, and without any fear of any but a confirmatory and satisfactory result, the trustees of the college made application to the Attorney General of the State for his official opinion upon the status of the institution. This was readily obtained, and was promulgated in the annual announcement of the college for the year 1876. There was now nothing more to say, and those who had been most ferocious in denouncing the American Veterinary College, and confident in predicting its early dissolution, became mute, and the college was permitted to enjoy in comparative quiet its subsequent and successful life, and to congratulate itself upon its recognition by the Regents of the University, its increasing classes of students, and its slowly enlarging alumni association.

This comfortable condition was undisturbed for a few years, but could not last forever. There was a cotemporary institution

which had from the birth of the American Veterinary College proved itself an envious and jealous enemy, and was industriously occupying itself in insulting the new and flourishing school, and seeking to disparage the value of its diplomas. They professed to hold that though the "school" might have been "incorporated," it still was not "chartered;" the "general law" was no law to organize under; a "special" act of incorporation was the only proper one, and no other could guarantee a valid diploma; no veterinary surgeon could be considered "regularly" graduated if he had only a American Veterinary College diploma. And much more of that sort, to the extent of their malice and ingenuity. Then came the suit against the United States Medical College, and the decision against it, declaring the illegality of its existence and organization, as determined by the decision of the Court. This was quite too grand an opportunity to leave unimproved. The American Veterinary College had been neglected and tolerated too long; the diplomas it issued must be again attacked, and, if possible, invalidated, by words at least, though not by resorting to the machinery of the courts, and reaching for a judicial decision, which was a cause for a procedure which might involve some other parties in unpleasant results.

Public announcement that there is but one school authorized by "special act" to grant diplomas, having been freely and industriously promulgated, and acting upon the theory that persistent pounding on one spot, however light the impression made by each blow may be, and though the result may not be perfectly satisfactory, will yet inevitably leave some marks, the Alumni Association of the American Veterinary College, probably stimulated by some ill-disposed members, began to fear that there must be some element of truth in the persistent and slanderous assaults made upon the standing of their alma mater, and one of them was induced to institute a legal suit to test the value of his degree. But a favorable decision in this case, though readily granted, was not likely to be satisfactory to the Board of Trustees of the College. They have honestly carried on their work for the last eleven years, and they feel that it cannot fail to be recognized and appreciated. Their act of incorporation is legally sound in every sense of the

word, and the decision of the Court of Appeals in the case of United States Medical College cannot apply to the American Veterinary College. The proper test, and the only one which could satisfy, must be that which they have sought from the fountain of authority, the Legislature of the State, and this has been obtained in the form of an act of which we print the text in the present number of the REVIEW. This act was passed by the Assembly on the 12th, and by the Senate on the 15th, and received the signature of the Governor.

The work of the American Veterinary College, as incorporated under the general law in 1875, is now sanctioned, reaffirmed and recognized by *special act* of the Legislature of 1886.

What will be the next step in advance of the constantly progressing history of that institution, remains to be seen.

HOG CHOLERA NOT ROUGET—WHAT IS IT?—Of all the discoveries which have resulted from the manifold labors of Pasteur, it has seemed to us that not one has promised to be of greater advantage to the agriculturist and the stock raiser, or, therefore, of greater interest to the veterinarian, than that which comprehends the prophylaxy of hog cholera. We could but reflect that if so simple an operation as that of double vaccination should be sufficient to protect the swine of the country from a disease which is costing our people millions upon millions annually in pecuniary loss, it was a matter which quite too largely affected the national wealth to be ignored or slighted.

What we have ourselves observed of the disease in Europe; the post-mortem examinations we have had opportunities of conducting in this country; the reports we have studied in the various agricultural papers; the light we had derived from the writings of Detmers, Law, Salmon and other investigators, concerning the nature, the etiology, the symptoms, and the lesions of the disease, whose views we had carefully collated and considered; had, we confidently judged, fully justified the conclusion we had reached, that the disease in Europe and in America was identical. Allowing for such minor differences as must be always looked for in the writings of different authors on the same subject, the general result seemed to be a settled one, and the natural

result upon our own mind was a strong and effective desire to secure the benefits for our own country which must needs follow the introduction and establishment of the plan of Pasteur, as not alone one among the prophylactic measures, but as *the* means, preeminently, of perfect prophylaxy in the case.

With this impression, we procured a supply of vaccine directly from the Pasteur laboratory, hoping for gratifying and important reports of its workings—to be sadly and effectually disappointed. It was tested under the best of auspices, portions having been furnished to the Bureau of Animal Industry in Washington and to the State Veterinarian of Nebraska, and the united testimony from both directions is of failure—disastrous failure in Nebraska, and worse than that in Washington. In Nebraska the result was negatively bad, simply in the fact of its being a failure as a prophylactic. But in the hands of Dr. Salmon it quite exceeded that, for the vaccine is not only inert as to preventive qualities, but may prove to be the agent of the introduction into the United States of a fatal swine disease, hitherto wholly unknown here, viz., that of *rouget* or *rothlauf*, the only disease amenable to the vaccine prophylaxy of Pasteur. In the series of letters which appeared in the *Breeders' Gazette* (not the REVIEW), and the publication of which we begin to-day, Dr. Salmon presents the public with the reasons he deems explanatory of the failure in his hands of Pasteur's vaccine. The nature of the disease is not that which it has been represented to be; in respect to the important points of its incubation, and its duration, and the post-mortem lesions, all is different, and of course under such conditions, nothing less than failure conld be expected, at the very best.

But if our hog cholera is not *rouget*, and not *rothlauf*—what is it? German and French investigators had made the announcement months ago that there were two diseases of swine, especially in Germany, equally fatal, and very similar in their symptomatology, but very different in their nature, at least as to their parasitic character, one of them being undoubtedly the *rouget* of the French or *rothlauf* of the Germans, the other being better known as *schweineseuche*. The important inquiry arises, whether

it is not this last-named malady which is destroying American swine; is not this the agency by which the Western States are losing millions annually?

This is truly an important question, and one also which unfortunately, cannot be decided to-day, though the worthy veterinarian of the Bureau of Animal Industry has attempted to give us the evidences that hog cholera is not *rouget*, in the remark, "that the microbe which produces the first is very different from the other; that the introduction of Pasteur's vaccine is not only useless but may contribute to the introduction and spread of a disease, the existence of which in this country has not yet been demonstrated," concluding his series of papers to the *Breeders' Gazette* by saying, "that he tenders the gratuitous observation that it would still be premature to jump to the conclusion that the German *schweineseuche* is identical with our hog cholera."

Our readers will find pleasure and profit in reading Dr. Salmon's letters. For better information on the subject, however, we would refer them to the second annual report of the Bureau of Animal Industry for 1885, recently published. This report contains a minute description of the investigation conducted by that section of the Department of Agriculture, and contains strong evidences of the intelligent manner and original views with which our worthy colleague seeks to throw light on the subject. Rouget, hog cholera, rothlauf or schweineseuche, whichever further investigation may prove it to be, is an important subject in a scientific point of view. If Pasteur's vaccine is a failure, cannot another be found? This is a gratuitous suggestion to those who are better acquainted than we profess to be with the delicate laboratory manipulations which such work demands.

Ganglionar Leucocythemia.—We give an interesting history, (with diagram), contributed by house-surgeon Dr. Walrath, of a post-mortem examination of a mare which was for a long time affected with this disease. The extent of the lesions, the peculiar appearance presented by the animal, and the fact that until the disease had reached a comparatively advanced stage, her functions were normally performed, her general condition continued to be good, and she was able to perform her work down to a period

within a few days of the time she was destroyed, will render the publication of the case interesting.

It is comparatively of recent date, or within only a little over twenty years, that this form of disease has been well observed amongst domestic animals, although the *Veterinarian* of 1839 contains a case recorded by Gulliver, which though not under that name, gives undoubtedly the evidence of its nature, viz.: an increase of the number of the white corpuscles. This condition of leucocytosis, it is true, is only a symptom, though varying in its character, well marked in some cases, where the disproportion is enormously great, while in others it is very much less remarkable. The usual normal relative average may be stated as 800, 900, 1,000 or 1,100 red to a single white corpuscle; of course not universally so. In the case recorded, the corpuscles numbered relatively, about one white to two hundred and sixty-five of the red blood cells. The difference has been sometimes noticed to be greatly in excess of this, cases being recorded where the figures stood one white to 85, 50, 46, 20, 15 and 12 of the red. The difference in man has been observed to be much greater, even standing 1, 2, 3 and 4 of the red to a single white. In all cases recorded, the diseased process existed, not only in the external, but also in the internal lymphatics, this case presenting this peculiarity, that none of the internal organs were diseased. All parenchymatous structures, such as the spleen, the kidneys, the intestinal, the heart and the lungs were in a healthy condition. The trouble was altogether limited to the external lymphatics of the whole body, and to a great extent represent a true case of *general cutaneous leukœmia, or ganglionar leucocythemia.* The literature on the subject of this affection as affecting domestic animals is comparatively limited, and amongst the cases recorded by Nocard, Griolet, Mauriz, Leblanc, in France, and Leisering, Bruckmuller, Siedamgrotzky and Bollinger, in Germany, none of this variety of case is found. So far as experience extends, this is the second case brought to our notice; another patient somewhat similarly, but not so extensively affected, was brought before the clinics of the American Veterinary College some eighteen months ago, but the case was lost sight of before a correct diagnosis, by *counting the blood* elements, could be accomplished.

MORTALITY OF HORSES IN NEW YORK.—We are indebted to the courtesy of Dr. S. S. Field for a statement of the number of dead horses received at the rendering dock of the city. The numbers were reported monthly for the first five years, and quarterly for the year 1885, and during these six years the number is found to aggregate 39,957. Further statistics would doubtless be interesting, were they accessible, and it is to be regretted that a more general and thorough statement cannot be obtained. The pathological statistics of the subject would be of interest. If we could learn more of the causes of mortality, and, aside from the ordinary forms of inflammatory diseases, could be advised of the number of animals which have died or were destroyed because of contagious diseases, it would largely assist us in devising measures of precaution against that class of maladies which without doubt contribute largely to the annual depletion of our equine population. In collecting the statement which follows, Dr. Field has certainly taken a step in the right direction. We hope he will continue his work, and, if he will accept our suggestion, and make application to the Board of Health for the necessary data, he may be able to improve his statistics greatly in the future, by giving us the number of glanderous and farcinous animals which find their way to the foot of Thirty-eighth street.

STATEMENT OF NUMBER OF HORSES RECEIVED AT RECEIVING DOCK, FOOT THIRTY-EIGHTH STREET, NORTH RIVER.

BY DR. S. S. FIELD.

	1880.	1881.	1882.	1883.	1884.	1885.
January	400	549	574	510	607	
February	417	564	606	451	545	
March	435	715	731	568	720	1,870
April	435	633	670	479	623	
May	436	537	546	469	541	
June	446	457	463	542	479	1,464
July	594	492	565	641	495	
August	511	599	545	517	497	
September	528	666	527	480	695	1,932
October	760	713	674	554	420	
November	765	726	496	494	394	
December	451	679	536	476	436	1,681
Total						39,957

REPORTS OF VETERINARY SOCIETIES.—State veterinary societies are comparatively new organizations in our country, and while some of them may claim for themselves a tally of years which quite relieves them from the stigma of being mere juveniles, there are others which really and consciously feel themselves to be still amenable to the charge of youthfulness, and allow that feeling to deter them from assuming what is in reality their proper position before the public. Hence their needless modesty in announcing themselves and uniting in current published communications and reports. The REVIEW has always been willing, and would take pleasure, to become the medium of intercourse between our professional brethren of every degree, and our pages are always open to any papers of value and interest which may be presented at the meetings of the various associations. In this, we have done nothing more than to fulfil a simple duty. As the principal organ of the veterinary profession on this continent, we have never ignored any proper request from any source. This having been our uniform course, it becomes a pertinent question to ask, whether it is not the proper thing for our American societies and brethren to take advantage of the hospitality we offer; and whether it is right to ignore an American publication and to seek that of a foreign country, when there seems to be a proper occasion for an appearance in the press? We feel this to be an injustice and an error. What takes place in the United States must necessarily be of greater interest to Americans than to English, French or German papers or people. We are prompted to these remarks by the reading of the reports of a Western society in one of the English journals. In crossing the Atlantic to find an organ that society has, of course, exercised a mere right which is exclusively her own, but would it not have been better to include the REVIEW in the courtesy of the same transaction.

If the Minnesota Veterinary Medical Association, or any other on the continent, will do us the kindness to remember us in the future, we will endeavor to treat them with the same justice and propriety which characterizes our intercourse with those who have accustomed themselves to consider the REVIEW as their own national organ.

THE "REVIEW" AND THE UNITED STATES VETERINARY MEDICAL ASSOCIATION PRIZES.—We publish in this number a paper on the etiology and pathology of parturient apoplexy in cows, presented by one competitor for the prize offered by the REVIEW in connection with that of the United States Veterinary Medical Association. This paper is the first that has come to us, and the first which is presented under the rules laid down for the competition. The committee on prizes of the United States Veterinary Medical Association, which consists of Drs. Corlies, Peabody and Johnson, will no doubt read it attentively, and we hope that our readers will carefully take notice of the article and be prepared to vote when the time comes. The paper is signed by "Incognitus." A sealed envelope, which we suppose contains the true name of the candidate, accompanies the essay, with the same motto on it outside, and has been placed in the hands of the secretary of the United States Veterinary Medical Association for safe keeping until the vote is known. The example is worth following, and we hope more applications will soon reach us.

ORIGINAL ARTICLES.

PARTURIENT APOPLEXY.

A Paper on the Etiology and Pathology of Parturient Apoplexy in Cows, respectfully submitted in competition for THE AMERICAN VETERINARY REVIEW Prize.

BY INCOGNITUS.

There is perhaps no disease to which bovine flesh is heir concerning the etiology and pathology of which there is less definite knowledge and greater diversity of opinion than parturient apoplexy. The various theories which have from time to time been formulated to explain the phenomena of the disease have, notwithstanding their inconsonance, numbered as ardent supporters some of the most eminent of bovine pathologists. The obvious result of this great diversity of opinion relative to the etiology and pathology of the disease, is that equally divergent modes of treatment have been practised; all of which, to the chagrin of the practitioner and loss of the stock owner, have proved most singularly unsuccessful.

In their writings on parturient apoplexy, as of many other diseases, veterinarians have to a great extent figured only as foster-parents. Now, while we fully appreciate the priceless value of the advantages our profession has derived from the accumulated knowledge of its elder sister, human medicine, and while the importance of the study of comparative medicine and surgery is obvious, still that the wholesale system of adoption practised by many writers on veterinary medicine has led to grave and unpardonable errors, and has been chiefly instrumental in bringing about a lack of independence of thought and original investigation, is, it appears to us, a fact too perspicuous and axiomatic to fail of recognition by the least observant.

The Traube-Rosenberg theory, "that eclampsia (in women) is due to œdema cerebri and the sequential anæmia," was adopted into veterinary medicine and ably supported by Franck and Fleming; while the one most generally accepted in human medicine, "that it is due to intoxication of the blood with the entire element of the organisms which go to make up the urine," was espoused by Dr. F. S. Billings, V.S., and vigorously supported in a paper read before the Boston Gyneological Society and published in the *Journal of Comparative Medicine and Surgery* for April, 1884. Before adopting these theories into veterinary medicine it was, as evidenced by the statement of Dr. Billings that "one cause must produce the phenomena in man and animals," assumed that eclampsia parturientum in women and parturient apoplexy in cows were analogous diseases. That there is some evidence to warrant that assumption is morally certain, but it is equally certain that that evidence is not sufficiently conclusive to justify such a dogmatic assertion as the one just quoted. Hence, it matters not one iota to the veterinarian which, if either, of those two theories be correct in human medicine unless it be shown beyond the peradventure of a doubt that parturient apoplexy in cows and eclampsia parturientum in women *are* analogous diseases. To our mind this has not been very satisfactorily shown, but since, as above stated, there is some evidence to support that view, we shall consider each theory as it applies to parturient apoplexy in cows, regardless of its

merits or demerits in human medicine, and leave until farther on the discussion of the question whether "one cause must produce the phenomena in man and animals."

The Traube-Rosenberg theory is founded on the *fact* that there is intercranial hæmal obstruction, the result of an altered condition of the circulation, produced by increased aortic pressure and the *supposition* that the resulting cerebral hyperæmia is always succeeded by œdema and anæmia. Many cases of active cerebral hyperæmia are not followed by œdema and anæmia; therefore to ascertain whether such be the case in parturient apoplexy in cows, the student must rely almost entirely on the clinical history and pathological anatomy. Inasmuch as Brown-Sequard has shown that epileptic convulsions are preceded by anæmia of the nerve centres, and Kussmaul and Tenner have proved, by numerous experiments, the dependence of convulsions on cerebral anæmia, is it not more than probable that if cerebral anæmia were the cause of parturient apoplexy in cows, convulsions would, at least, be a common, if not a constant, symptom of the disease? But it is a notorious fact that convulsions are rarely or never present; in fact we have never seen a case where genuine convulsions occurred, except during the last throes of death; but neither have we seen a case where spasms and deliriums were not present during the early stages of the disease. In judging of the occurrence of convulsions it is essential that they be carefully distinguished from deliriums and spasms, and consequently in default of that distinction certain writers mention convulsions as a symptom of the disease, while others remain perfectly silent respecting this point, leaving us to infer that they have never seen what they consider genuine convulsions. It is an incontrovertible fact that cerebral anæmia produces symptoms resembling to a degree those of parturient apoplexy, but it nevertheless appears that the paralysis and the profound and prolonged coma of that disease resemble more closely the phenomena resulting from active cerebral hyperæmia. Fleming says the "anatomical arrangement of the 'rete mirable' is such that it chiefly supplies the cerebrum; this should tend to explain why in eclampsia in the cow, the comatose symptoms are so com-

mon that convulsions seem exceptional"; while Dr. Billings says "the reason that convulsions are not so frequent or severe in cows, assuming the cause to be the same, is surely not to be sought as much in any peculiarities of the circulation, as in the very low degree of nervous irritability which is common to the bovine species."

Fleming founds his explanation on the belief that anæmia of the cerebrum is less likely to produce convulsions than anæmia of the medulla, and the erroneous conclusion that the "rete mirable" by its "anatomical arrangement" renders cerebral hyperæmia more likely to be followed by œdema and anæmia, whereas the fact is that this peculiar distribution of the vessels renders active cerebral hyperæmia less likely to be followed by œdema and anæmia; for effusion is more likely to take place where the maximum weight of blood pressure is quickly reached and the vessels rapidly distended to their utmost capacity. "The very low degree of nervous irritability which is common to the bovine species" would, to a certain extent, render convulsions less frequent and violent; but, "assuming the cause to be the same," it does not account for their entire absence, especially when it is remembered that, under other conditions, convulsions of a most severe type occur in cattle.

The reason why convulsions are seldom or never seen in parturient apoplexy in cows is to be sought more particularly in the etiology of the disease than in any peculiarity of anatomical arrangement or lack of nervous irritability in the animal.

A necropsy, having for its object the examination of the contents of the cranial cavity, should be conducted with the utmost precaution, for merely post-mortem meningeal hyperæmia or anæmia is no index of ante-mortem cerebral hyperæmia or anæmia, inasmuch as the condition of the meningeal vessels depends to a great extent "upon the position of the body after death, the length of time during which the blood remains fluid, the manner of dying, and whether the head or trunk is opened first at the post-mortem examination." (Flint.)

In view of the great importance of a correct knowledge of the pathological anatomy, it is a much-to-be-regretted fact that

very few veterinarians seem to have taken the trouble to thoroughly investigate the matter for themselves, and hence there is a very great dearth of literature pertaining to this phase of our subject. But, notwithstanding this dearth, there is, taken in connection with our own observations, sufficient to convince us that the cerebrum is in a hyperæmic condition, and that anæmia of the brain is rarely present even in the last stages of the disease.

As might well be expected, the urine intoxication theory has been treated with cold indifference in the domain of veterinary medicine, but since it is the one commonly accepted in human medicine, and since a large number of veterinarians are inclined to the belief that the same agencies produce the phenomena in both man and animals, it is certainly worthy of careful consideration. If this theory be accepted, it must also be admitted that in parturient apoplexy in cows, as in eclampsia parturientum in women, anuria as well as albuminuria precedes the attack. In regard to this matter Dr. Billings says: "The secretion of urine generally diminishes early in the attack; anuria is frequently present. The urine contains albumen and casts. Franck says he has frequently observed albumin to be present in the urine of cows antecedent to parturition.

"Veterinarians do not seem to have given sufficient attention to this condition of the urine, a circumstance which must necessarily lead to mistaken views as to some of the complications, in fact that which we consider the chief one of the disease.

"That albuminuria is present in nearly every severe case is unquestionably true, yet so practical a writer as Williams says: '*The urine is pale in color and free from albumin; the secretion is retarded.*'

"Fleming says: 'Micturition is also, as a rule, suspended from the commencement; consequently urine accumulates in the bladder. When speaking of treatment he also says: 'The urine should be frequently removed from the bladder.'

"Here are two errors, one due to false translation; the other to want of practical experience, or perhaps forgetfulness.

"Micturition means passage of urine. Mr. Fleming should

have translated Franck's words as follows: 'The secretion of urine is generally stopped, anuria; in some cases, it is only partially suppressed, however.

"Urine does not accumulate in the bladder, that organ being frequently almost empty for hours; hence removal of its contents is unnecessary unless at an early stage, or in order to examine it. On account of the paralyzed condition of the patient it is a question if the urine would not flow out of itself, the position of the animal favoring it, when the bladder overfills. Spinola says, 'that it frequently does.' I have never seen a bladder so distended, even in the early stages, that it was necessary to empty it for the comfort or health of the animal."

These remarkably inconsistent statements afford but another illustration of how the shackles which the teachings of our predecessors, our own preconceived ideas and long cherished theories are apt to clasp upon us, may lead us into the error of mistaking effect for cause.

No sane man at all acquainted with the clinical history of the disease would for an instant deny the presence of anuria any more than he would that of paralysis, but neither of them is among the first manifestations of the disease; therefore the former, as well as the latter, is a result and not the cause of the disease; for it is impossible for urine intoxication to be the cause of a disease the beginning of which is not preceded by a diminished secretion of some element of the urine.

We would not for an instant attempt to calculate the "practical experience" of any writer from a single assertion, but if Dr. Billings has "never seen a bladder so distended, even in the early stages, that it was necessary to empty it for the comfort or health of the animal," his has been, to say the least, a most remarkable experience. We have never seen a case where catheterization was not necessary both for the health and comfort of the patient, except where micturition had been accomplished shortly before the recumbent position was assumed, and this is strictly in accord with the experience of a larger number of practical veterinarians, a list of whom we have before us and among whose names appear those of such men as Fleming, Delwart, St. Cyr, Cruzel,

Williams, Hill, Steel, Smith, Liautard, Michener, and many others of prominence. And, moreover, it is an established scientific fact, applicable to the cow, as to all other mammalia, and explicable by perspicuous physiological axioms, that during the forty-eight hours immediately following the act of parturition the secretion of urine is greater than during any other equal period of health. In the first place, this fact fully accounts for the distended condition of the bladder at the time the practitioner is called, and in the second place, in view of the fact that parturient apoplexy seldom occurs within twenty-four hours after parturition, *it precludes the possibility of the blood being "intoxicated with the entire element of the organism which go to make up the urine."*

Taking for granted that, as Billings says, nearly every severe case of parturient apoplexy in cows is accompanied by albuminuria, and that, as Franck says, it is often present antecedent to parturition, that proves absolutely nothing in regard to the etiology of the disease. In order to prove the dependence of a disease on albuminuria, it is not necessary to prove that it follows every case of albuminous urine, but it is necessary to prove that every case of the disease be preceded by albumin in the urine; and in the disease under consideration even more than that is necessary, for very nearly if not quite every case of advanced pregnancy is accompanied by a varying increase of albumin in the urine, the quantity of which is probably augmented by an attack of parturient apoplexy and possibly the more severe the attack the greater that augmentation, but the quantity of albumin in the urine previous to parturition affords evidence of the probability of an attack of parturient apoplexy only so far as it indicates the extent of the physiological changes which take place during the latter stages of utero-gestation. Albuminuria is a result of advanced pregnancy and perhaps parturient apoplexy, and no more the cause of the latter than of the former.

The foregoing opinion, relative to the presence of albumin in the urine of pregnant cows and its importance as a factor in the etiology of parturient apoplexy, is based on personal observations and the following physiological laws which favor the production

of albuminous urine at that particular period: first, an increased activity in the circulation owing to a physiological hypertrophy of the heart; second, an increased quantity of blood, thereby producing an increased vascular fulness; and third, an increased amount of fibrin factors accompanied by a diminished proportional amount of corpuscles and albumin in the blood.

The question, are eclampsia parturientum in women and parturient apoplexy in cows analogous diseases produced by analogous causes is briefly presented in the following table of comparison:

WOMEN.	COWS.
About eighty per cent. of the cases are in primaparæ.	There is no proof that it ever occurs in primaparæ.
It is generally in connection with a difficult parturition.	It seldom or never occurs in connection with a difficult parturition.
It very frequently manifests itself during delivery.	It seldom or never manifests itself until from twenty to thirty-six hours after delivery.
Convulsions are the chief characteristic of the disease.	Convulsions are seldom or never present.
The temperature of the body is above normal.	The temperature of the body is below normal:
The urine contains albumin both before and during the attack.	The same in cows.
The bladder as a rule contains but little urine.	The bladder as a rule is more or less distended with urine.
There is functional inactivity of the kidneys both before and during the attack.	There is increased functional activity of the kidneys before the attack, but functional inactivity of those organs, as well as of many others, after coma sets in.
There generally is œdema of the face and extremities.	There never is œdema of the face and extremities.
The head is frequently drawn to one side, said to be due to contraction of the muscles of the neck.	The head is almost universally thrown round on the shoulder, also, but very erroneously, said to be due to contraction of the muscles of the neck.
A post mortem examination reveals anœmia of the great nerve centres.	A post mortem examination reveals hyperæmia of the great nerve centres.
During the convulsions the pulse is weak, quick and often intermittent, but as the convulsions subside the pulse becomes slower and stronger.	The pulse is at first generally full, soft and slow, but subsequently becomes quick, small and wiry, and finally imperceptible.

To briefly recapitulate it will be observed that in women a large percentage of the cases are in primaparæ, generally in connection with a difficult parturition, and most frequently during delivery and especially in the periods of dilatation and expulsion; while in cows it never occurs in primaparæ, nor in connection with a difficult parturition, and it is seldom seen within twenty-four hours after delivery. Again, in women there appears, in a majority of the cases, to be a direct connection between the act of parturition and the disease, great and general irritability of the nervous system, and more or less rapidly recurring convulsions; while in cows there is no direct connection between the act of parturition and disease, the general irritability of the nervous system is entirely wanting," and genuine convulsions are seldom or never seen. Robust, young, and nervous women, and mature, plethoric, and deep milking cows ars more liable to this affection than those of opposite classes; in fact a plethoric maturity seems to be an essential factor in the etiology of the disease in cows, while in women the disease, in its nature, appears to be essentially a reflex neurosis, probably resulting from stenosis.

Notwithstanding the universally accepted dogmas of eminent veterinarians to the contrary, we are by the foregoing facts irresistably drawn to the conclusion that eclampsia parturientum in women and parturient apoplexy in cows are entirely different diseases, produced by different causes, and presenting very diffierent phenomena.

The theory which, above all others, is least sustained by facts and most purely the work of the imagination is that propounded by Barlow and vigorously supported by Williams, and of which the latter says: "The arrest of the lactiferous secretion is doubtless due to some disturbance of the organic system of nerves, but how this arises is difficult to determine unless we take into consideration that the great sympathetic is developed to a greater extent in deep milkers than in other cattle, and that in consequence it is more susceptible and more prone to derangement from trivial causes."

Where the lactiferous apparatus is more highly developed the logical inference is that the portion of the sympathetic nervous

system supplying that apparatus is also more highly developed, but that the whole sympathetic nervous system is more highly developed in consequence of this increased development of a certain portion of the system is most assuredly an illogical inference. Williams is not only wrong in his premises but also in his reasoning, for if it be granted that the whole sympathetic nervous system is more highly developed it does not follow that it is "more susceptible and more prone to derangement from trivial causes."

An increased functional activity without a corresponding increase of development would, most certainly, render an organism more liable to derangement from trivial causes; but an increased functional activity, provided it be accompanied by a corresponding increased development, does not, by any means, render an organ "more susceptible and more prone to derangement from trivial causes.'" Well bred cattle are, as a rule, of a more nervous temperament, but that is independent of any increased development of the lactiferous apparatus.

It is also stated that whereas parturient apoplexy follows an easy delivery the amount of nerve force provided by nature for the accomplishment of the act of parturition is not expended and that the surplus of nerve force thus remaining, by some means, deranges the sympathetic system of nerves and thereby produces the disease.

The *ridiculosity* of such an explanation is too apparent to render it worthy of serious consideration.

That the functions of the sympathetic nervous system are deranged in parturient apoplexy appears quite evident, but that that derangement is brought about by an increased development of the system, or by a surplus amount of nerve force is, to say the least, decidedly improbable.

The theory of Dr. J. H. Cox, that parturient apoplexy is due to thrombi being formed in the cotyledons of the uterus, and that the phenomena of the disease are due to embolism and its effects is as inconsistent with facts as it is ingenious, for thrombi formed in the uterus, if they enter the circulation at all, must of necessity enter it by the uterine veins and in such a case

is it not reasonable to infer, nay, is it not absolutely certain, that they would be obstructed in the lungs and that pulmonary, not cerebral apoplexy would be the result and the primary lesion? And moreover, it might be pertinently asked, does the uterus always contract forcibly and quickly, and are its vessels always "in that pristine condition necessary to prevent the admission of thrombi into the circulation," in old and debilitated animals, in poor milkers, and after a difficult parturition?

The theory of cerebral hyperæmia, or rather the Traube-Rosenberg theory as adopted and supported by Franck and Fleming, except that the cerebral congestion is not followed by œdema and anæmia, is certainly worthy of careful consideration, inasmuch as it is sustained by the clinical history and pathological anatomy and explains more fully and satisfactorily, than any other yet propounded, the complex phenomena of the disease.

The cerebral hyperæmia is a result of increased aortic pressure and cerebral excitement consequent on the act of parturition. The increased aortic pressure is produced by a physiological hypertrophy of the heart and an increased vascular fulness which are always present in advanced pregnancy; by the excessive plethora so common in deep milking cows when dry or nearly so; and by the large quantity of blood, required by the uterus during the latter stages of utero-gestation, being thrown back on the system after parturition.

The fact that parturient apoplexy is more common in the best milking strains of well bred cattle is explained by the fact that they are, as a rule, better cared for and that deep milkers become much more plethoric when dry or nearly so, as just before calving. It seldom occurs before the third and after the seventh calf, because between those periods the animal has reached maturity, but has not yet begun to decline, and it is therefore more disposed to plethora, while the act of parturition occasions less constitutional shock and exhaustion.

The cow is rendered more susceptible to the disease than other animals because by domestication and careful breeding she has been converted into a mere milking machine, as it were.

The reasoning which says that were the mental excitement

consequent on the act of parturition and the removal of the offspring a predisposing cause of the disease, then it would occur more frequently in primaparæ, is, to say the least, both narrow-minded and absurd, for other conditions are by no means equal, inasmuch as at the first calf maturity has seldom been reached and even where it has, the exhaustion is much greater, and the plethora less than at a subsequent calf.

The chief arguments used against the theory of cerebral hyperæmia are "It's too simple" and "I don't believe it," but since in medicine facts are king and mere belief or disbelief counts for nothing, and science is but the systematic classification of established and self-evident facts, such arguments are not worthy of serious attention.

The foregoing comprises a short review of not only the most recent theories formulated to explain the complex phenomena of parturient apoplexy, but also of those theories most generally accepted by competent veterinarians, and therefore, although they be innumerable, those remaining unnoticed are not of sufficient importance, having long since been exploded, to warrant the prolongation of an already somewhat lengthy paper.

It has been stated that it requires much less intellectual worth to criticise than to construct a theory, but that depends entirely on whether the theory constructed be imaginative or in accordance with "the cold logic of facts." In consequence of neither possessing a high degree of intellectual ability, nor being of an imaginative turn of mind, we have neither aimed at presenting anything startlingly new nor profoundly wise. But in the study of our subject, knowing that too frequently conclusions are formed from preconceived ideas, important facts being taken entirely out of their proper relations to support perchance a theory which has little to sustain it but the enthusiastic exuberance and ingenious imagination of the individual in whose mind it originated, while scarcely less important facts are completely ignored for the same irrational purpose, we have endeavored to collect and classify according to their importance and relations to one another all the obtainable facts relating thereto, from which data, inasmuch as we have not been in-

fluenced by any desire to rear some original pet theory, our conclusions have been cautiously and impartially drawn. We have not, however, been oblivious of the fact that popular prejudice is somewhat disposed to deem it presumption in a younger member of the profession to doubt the views entertained by his seniors, especially when he essays to place those doubts on paper, and therefore our defense is that, having carefully examined the various hypotheses advanced by the different writers on the subject, in our deliberate judgment there are excellent reasons for seriously doubting the soundness of the views generally entertained by the profession; and such it has been the object of this paper to show, in the hope that it might aid in stimulating more competent members of the profession to greater exertions and thereby expedite the settlement of this much vexed question.

INCOGNITUS.

THE ÆTIOLOGY OF RABIES

AND THE METHOD OF M. PASTEUR FOR ITS PREVENTION.*

BY HERMANN M. BIGGS, M. D.,
Instructor in the Carnegie Laboratory.

The ætiology of rabies and the investigations of the cause or causes of the disease and of the conditions which predispose to its development or act directly in its production, form subjects of inquiry of the greatest importance. Upon the solution of these questions depends largely the determination of the methods to be sought for in the prevention of this disease, which is perhaps the most terrible one to which the human or animal organism is subject. Much study has been given to these questions, and among the writers who have made investigations in this direction may be found many of the most distinguished veterinarians of every generation. These investigations, however, have resulted only in the presentation of many hypotheses as to the nature and character of these causes. They have not established a single

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condition or circumstance which can bring about or predispose to the development of the disease, excepting only the contagious principle as transmitted from rabid animals. The origin of the malady in the canine and feline species is, in many instances, involved in the greatest obscurity, and, being unable to account for these cases upon any other supposition, many observers have believed in the occasional spontaneous origin of the disease. Fleming, the ablest of the recent English writers on this subject, says: "And rabies, we feel we are justified in asserting, must also be included in the list of diseases with glanders, anthrax, typhus in pigs, and the distemper in dogs, which, generated under certain favorable conditions, during their course develop a virus that, like a ferment, acts as a leaven in producing morbid and characteristic changes when it obtains admission to the previously healthy body, and there are few nowadays who are not convinced that it will occasionally appear in a spontaneous manner and without any exciting cause."

The most potent arguments which have been brought forward as proving the spontaneous development of rabies are: 1. The appearance of the disease in an epizootic form in countries where it has been previously unknown, and where its appearance could not be traced back to any foreign source. The epidemic of the disease in Peru, in 1803, may be cited as an instance of this kind. 2. It is urged that the appearance of the disease in an epizootic form in some countries at rare intervals can also only be explained in a satisfactory manner on this hypothesis. It has been observed, not infrequently, that, in certain countries which have been free from the disease for long periods of time, suddenly it appears in almost an epidemic form, and, after a short time, becomes again apparently extinct. Dr. Roucher, as quoted by Fleming, refers to the appearance of the malady in Algeria as confirmatory to some extent of its spontaneous origin. The marked intermittence in its manifestation at different geographical centers; the multiplicity of cases at certain epochs of very short duration, contrasting strikingly with their rarity at other times; the intervals between the outbreaks, which are longer than the period of incubation of the virus; the small number of cases as

compared with the crowds of wandering dogs; the difficulty of explaining the maintenance of the disease in every region after long interruptions—all this would lead to the supposition that inoculation is not the sole cause in Algeria.

But whatever disagreement there may have been among writers as to the spontaneous origin of rabies, there has always been the greatest unanimity as to its contagious nature. This important fact has been universally recognized by the most competent medical writers from the very earliest times. But more recently, we may say, it has been almost fully established that this contagious principle, whatever may be its nature, is the only "evident, efficient, and incontestable cause" in the production of this malady. The occasional spontaneous development of the disease in certain of the carnivora, as advocated by many veterinarians, must be entirely rejected, in view of the facts recently demonstrated with relation to the germ diseases, and we must accept absolutely the conclusion that "this disease is maintained and spread solely by its contagious principle, and that there are no other causes in operation."

The facts adduced by Roucher, Fleming, Tardieu, Bouley, and others, to prove the spontaneous appearance of the disease, may be readily and rationally explained on the ground of its purely contagious nature, and the arguments brought forward to sustain the former view may be easily refuted. It is a matter of the greatest importance that those fallacious ideas should be corrected which are so prevalent among the laity, and to no small extent among the profession in regard to the influence of climate, season, hunger, thirst, food, pain, anger, an unsatisfied sexual desire, and the thousand other causes that have at one time or another been adduced as predisposing to or producing the disease in dogs. There are no better or more reasonable grounds for believing that any one or all these influences combined can bring about the development of a single case of rabies than that unfavorable sanitary conditions can produce small-pox when the specific contagium vivum of the disease is not present. So strong has been the popular belief in the effects of the season in the production of rabies, that not even the testimony of indisputable facts has been

potent enough to dispel it. It has been almost a universal belief since the earliest times that the canine species are more susceptible and liable to an attack of this malady during the hot weather than at other seasons of the year, and for certain periods during the summer and autumn months many precautions have been taken to protect man against the possible dangers arising from this cause. During this period we find that in many cities there are municipal ordinances requiring that all dogs at large in the streets shall be provided with muzzles, while during the remainder of the year they are left free. The danger from the possible development of the disease has been supposed to be proportionate to the intensity of the heat. This supposition has been maintained in spite of the fact that it has been shown in the most unmistakable manner, by large masses of statistics collected with the greatest care in France and Germany, that the disease is not more prevalent in the summer months than at other seasons of the year, and that, if there is any preponderance of cases in any season, it is rather in the winter and spring than the summer. Indeed, Dr. Röll, of the Vienna Veterinary Institute, asserts that the disease is more frequent in mild than in hot summers, and that the only exceptions have been when the seasons were irregular or variable.

It has also been a popular superstition that the bite of an angry dog, or of one in rut, or of some other enraged animal, was likely to be followed by rabies. Fleming reports two cases mentioned by the Abbé Rozier, where hydrophobia was supposed to have followed bites of this kind. In the first case a man had bitten himself in a fit of rage, and in a second a soldier was bitten by his comrade. Dr. Camille Gros, as quoted by Fleming, records a case that occurred in the clinic of M. Tardieu which has been cited as proving the same fact. "An individual was bitten by an angry but not rabid dog. The wound healed in ten days. Afterward the subject became ill, symptoms of hydrophobia manifested themselves, he was brought to the hospital, and died in four days; at the autopsy the lesions present in hydrophobic patients were found. The premonitory symptoms and the progress and duration of the malady were the same as with them. From

this fact this author supposed that the bite of an irritated dog or one in rut might become momentarily virulent and cause rabies." However, this supposition is utterly untenable, and certainly nothing can be more erroneous or terrifying than the idea that the bite of a healthy dog may, under certain conditions, be followed by rabies. A wound or injury produced by the teeth of a non-rabid dog—one whose saliva does not contain the living organism, whatever may be its nature, which is the cause of rabies—can no more bring about the production of the specific disease which we call rabies than can the wound made by a sterilized knife.

This, with all other supposed causes, must be absolutely rejected. It must be admitted that there is only one evident and efficient cause for the disease—a contagium vivum—as to the nature or character of which we have at present no absolute knowlege, excepting that it must be a living matter capable of reproduction and multiplication when transferred to the living organism, and of there producing a specific disease manifested by varying symptoms in the different species of animals affected, and its presence or virulence can only be determined by the results produced when introduced into the blood of previously healthy animals. That it is exceedingly minute is proved by the difficulty experienced in its demonstration, and that it is a micro-organism belonging to the schizomycetes is rendered probable by the similarity of the disease to other communicable diseases known to be due to pathogenic micro-organisms of this class. Indeed, Dr. Herman Fol, of Geneva, in a recent communication to the French Academy, professes to have discovered the specific micro-organism of this disease—to have been able to demonstrate it in the neuroglia of the central nervous system—to have succeeded in cultivating it in appropriate cultivation media, and to have produced rabies in rabbits by inoculation with pure cultivations of this germ. In this communication he describes the material points in the results obtained in his experiments and the methods employed as follows. He says: "Like our predecessors, we have tried in vain to obtain a staining of some special organism in the cords from rabid animals by the ordinary methods, but

finally by the adoption of one method we have been able to reveal the existence of certain elements which are not present in healthy spinal marrows. This result was attained by the use of the method of hardening and coloring devised by Ehrlich and Weigert, and by making an absolute rule to examine only perfectly satisfactory sections, whose thickness did not exceed $\frac{1}{100}$ of a millimetre.

"The marrows should be immersed immediately after death in a solution composed of 2·5 grm. of bichromate of potassium and 1 grm. of sulphate of copper to 100 grm. of water. The sulphate of copper is important for the subsequent staining, and also because of its antiseptic properties, which guarantee that no new organisms gain access to the section during the process of hardening. The cord is afterward divided into short sections, so that they can be immersed in Weigert's hæmatoxylin solution; they are then passed into absolute alcohol and oil, imbedded in paraffin, and a series of fine microscopical sections is made. These are then passed into Mayer's fluid, decolorized in ferrocyanide of potassium, and mounted in Canada balsam. Similar pictures may be obtained, but less clearly defined, by subjecting the small sections of the cords to the vapor of osmic acid, and decolorizing in a solution of oxalic acid before imbedding.

"If these preparations have been decolorized with care, there may be seen in them groups of small globules similar to micrococci. These are situated sometimes in the lamella of the neuroglia, more rarely in the space in the cylinders between the portion stained deep blue and the sheath of Schwann, which is only tinted yellow. Sometimes these groups are found in the small cavities, which have a diameter less than that of the myelin fibers, the histological nature of which is not as yet understood. These granules are perfectly spherical, clearly defined, and stained a deep violet color. They are arranged in a definite order, and do not form chaplets. They are found in the form of a figure of eight (8), which indicates multiplication by division. Their diameter is about 0·2 of a micro-millimetre.

"If an appropriate cultivation medium is inoculated with a rabid brain, a development occurs, accompanied by a slight cloudi-

ness, which settles to the bottom on the fourth day. This deposit, when used for the inoculation of sound animals, produces in some instances a rabies which is very characteristic, excepting that the duration of the incubation period is much more prolonged than that following the employment of the virus which served for the original inoculation."

As a cultivation medium, a brain broth was used, usually that from sheep, as fresh as possible, and triturated with a little sterilized water and carbonate of potassium. The liquid, filtered at first through paper, is then passed through a Chamberland filter, and after this remains perfectly clear if all the operations have been carried on with sufficient care.

The method of trephining is not used, as being too complicated. The virulent liquid is injected by a pointed cannula carried along the conjunctiva to the bottom of the orbit, which is then made to pierce the lamella of bone which separates the base of the brain from the orbit. This method, according to Fol, has succeeded well, and is easy of application.

If a portion of the deposit which the cultivations present after four days is placed on a cover-glass, dried, and treated with the hardening solution, composed of bichromate of potassium and sulphate of copper, and is then colored and decolorized in the same manner as the sections of the cords, the same groups of micrococci are presented, and they have the same deep violet color. Rabies did not follow inoculations with cultivations more than six days old. He says: "It would be interesting to know whether this depends upon an attenuation of the virus, and whether the inoculated animals thus become refractory to rabies."

Pasteur had before described the presence of certain granules in rabid marrows; but Fol says: "In the want of precise descriptions, it is not possible for us to decide whether they are identical with the microbe which we have succeeded in coloring and cultivating. And as for the brilliant granules described by M. Gibier, they appear to be much larger than this microbe, which is not visible with a magnifying power of five hundred or six hundred diameters."

These statements of Fol were not favorably regarded in Pasteur's laboratory, on the grounds, first, that the methods which he describes as used for the cultivation of the germ have been repeatedly employed by M. Roux, Pasteur's first assistant, without any result; second, that he describes the germ found by him as being aërobic, while, from the nature of rabies and the presence of the contagium vivum in the nervous system, it seems far more probable that it is an anaërobic or facultative anaërobic germ; and, third, because he has only produced rabies in rabbits, in which the disease manifests itself by no characteristic symptoms, as is the case in dogs, and in which rabies may be easily confounded with the various forms of septicæmia.

The studies of M. Pasteur upon rabies up to the time of the meeting of the International Medical Congress in 1884 are well known, through the communication presented at that time, and through numerous previous communications to the French Academy. It is scarcely necessary or possible to consider these communications in this short paper, as they in large part report observations which have no important bearing on the present question, and present lines of investigation quite different from that described in his last report. However, it is but fair to state that some of the conclusions arrived at in his earlier work on rabies have not been confirmed either by his own later investigations or those of other observers, and are in some respects opposed to the results recently reported. It must be admitted also that, unfortunately, in his last communication, as in some of his other more recent communications upon rabies, he has not discussed the questions so much in detail as could be desired, nor has he given definitely the number or character of the experiments upon which his conclusions and the general principles drawn from them are based. The failure to do this will render it impossible for any other investigator to repeat or confirm his experiments, and without such confirmation from a thoroughly reliable source there will of necessity be much reserve among scientific men about the acceptance of such important and far-reaching principles as are involved in his method for the prevention of rabies.

It may not be uninteresting here to first recall briefly the former method of Pasteur for rendering dogs refractory to the disease, as presented in his communication to the French Academy on May 20, 1884. This first method was, briefly, as follows: The rabic virus obtained from the spinal cord of a rabid dog was first attenuated by passing it through a series of monkeys, and then, as the virulence is increased again by its transmission through rabbits, it was graded up to the desired degree of virulence by the subsequent inoculation with it of one or more rabbits. In rendering dogs refractory to rabies they were first inoculated with a virus obtained from the *first* of the *rabbits* inoculated from the *last* of the series of *monkeys*. At the same time with the inoculation of the dog a *second* rabbit is inoculated. The dog receives his *second* inoculation from the virus obtained from the *second* rabbit, and at the same time a *third* rabbit is inoculated with this same material. The *third* and last inoculation of the dog is made with the virus obtained from the *third* rabbit. "At this time," Pasteur says, "the dog has been rendered entirely refractory to rabies." In this same communication, in 1884, he says: "I believe I can surely bring about a refractory state in the human being before the mortal disease appears in consequence of a bite."

(*To be continued.*)

VENEREAL DISEASES IN THE LOWER ANIMALS.

Read by Professor WALLEY before the Scottish Metropolitan Veterinary Medical Society.

MR. PRESIDENT AND GENTLEMEN.—In bringing before your notice the subject of "Venereal Disease in the Lower Animals," I may state at once that I have several objects in view. *The first of these is to inquire as to what extent our domestic animals are liable to such infections. The second is as to whether we are justified, in the present state of our knowledge, in assuming that they suffer from conditions allied to those which are characteristic of syphilis in man. The third is to direct attention to a method of treating intractable venereal sores in the dog which is both sure and effectual.*

In reference to the first of these questions I may remind you that veterinary practitioners in this country are well acquainted with the fact that several of our domestic animals are sometimes affected with the disease known as *gonorrhœa* or, as it is termed in cows, "bull-burnt."

This specific form of urethritis, for such it is, occasionally makes its appearance in herds under such circumstances as to warrant the expression of an assumption that it is in such cases generated autogentically.

I have myself seen it appear in herds in which neither the male nor the female members had ever been allowed to come in contact with other animals for procreative purposes, and I am well aware that other practitioners have seen it arise under identical conditions. In the matter of the dog I have frequently had cases brought to me in which the owners of the animals have declared that they (the dogs) had *never been in contact with any female for copulative or other purposes.* When once the disease arises in the male its spread is not difficult to explain, and I have notes of some cases in a number of mares which came under the observation of one of my late pupils, Mr. Barclay, of Dunfermline, and which were all traceable to infection from one stallion, who, it was subsequently shown, was the subject of the malady.

In France there exists a well-known form of venereal disease, known as "La Maladie du Coit." Happily—at least, so far as I know—the equine species in this country is exempted therefrom. Neither have I ever seen any such form of disease in the sheep or in the pig.

As to how far gonorrhœa may be looked upon as a specific disease, I may first direct your attention to the fact that the literal meaning of the word used to designate this class of affection is totally out of accord with its proper application. It signifies an excessive flow of semen—a condition sometimes observed in young vigorous dogs when excited by the near proximity of œstral bitches, and which, owing to the fact that it is accompanied by tumefaction of the sheath and swelling of the testes, is confounded with the affection usually termed gonorrhœa. The use of the term is, however, warranted by custom and I do not feel disposed

to cavil at its application to the form of disease under consideration.

As to the specificity of gonorrhœa, I may remind you that the researches of those engaged in inquiries as to the nature of the disease in the human subject have not resulted in any definite issue. True, the statement has been made that a micrococcus has been detected in the discharge of gonorrheal urethritis, and in those of gonorrheal ophthalmia, identical in its characters from both sources; but there are those who assert that in urine which has undergone fermentative changes a micrococcus, recognized as *M. urinæ*, is always present; further, inquirers—MM. Lepine and G. Roux—have recently drawn attention to the fact that when cultivations of this now well-known micrococcus are introduced into the urethral canal of male animals, and retained there for a short time, a specific urethritis is established, which extends to the bladder and kidneys, and produces death, with reproduction in large numbers of the micrococcus in the urinary products; and of still greater importance is the further statement that healthy females confined in cages with these experimentally-infected animals become the subjects of identical conditions, and die with similar urethral, cystic and renal lesions well developed.

Now, if these altered micrococci were the actual cause of gonorrhœa, we should expect that in old-standing cases the bladder and kidneys would become diseased; such, however, is never observed—at least in my experience. That specific urethritis in animals is identical with that of man is proved by the fact that in dogs we not infrequently see well-marked gonorrheal conjunctivitis produced by contact of the urethral discharge with the conjunctivæ. The infection is conveyed by the animal licking its penis, and subsequently one of the hind feet, from which the virus is passed to the conjunctiva in the act of scratching. Mr. Alex. Grey informs me that some years ago he saw a monkey—which had contracted the disease from handling contaminated lint thrown away by a sailor on board ship—with gonorrheal conjunctivitis, the infection having been conveyed to the membrane by the creature's own hand.

So far, then, we see that gonorrhœa affects the horse, the ox,

the dog and the monkey; that in these animals its course is similar to that which follows in man. But what about our second query: "Do the domestic animals suffer from syphilis?"

The great characteristics of syphilis are—firstly, that it is a disease running a chronic course; secondly, that its most prominent lesions are indolent ulceration of mucous membranes and skin, with chronic enlargement and induration of lymphatic glands and ulceration of the skin covering these glands.

Do we meet with such lesions in veterinary practice? Most certainly yes—in the dog at least. Are they of a syphilitic nature and origin? I will answer this question by expressing the opinion that they are of that nature, and I think I am warranted in so doing by the characters presented by these lesions and by their course.

I am quite aware that objection may be taken to my conclusions on the ground that the chronic lesions seen in animals are only results—or *secondary symptoms*, if you like—of gonorrhœa. So used pathologists to consider the lesions of syphilis in man; but they have now come to look upon them as being totally different both in nature and in cause.

I know it has been said that the *chancre* of the dog differs materially from that seen in man, in the fact that its edges and base are less indurated. Granted this to be the case, may not, I ask, any slight difference in character be due to differences in structure (histologically) in the membranes in the two animals?

A chancre makes its appearance on the penis of the dog; it gradually extends its bounds; other chancres form, and in some instances the primary and secondary sores coalesce; from the surface of these sores a puriform fluid, ichorous and infective in its character, is discharged; this fluid sets up a degenerative inflammation in the skin of the thighs, the abdomen, the chin, and in fact in all parts with which it comes in contact. These sores do not yield to the local application of caustics, and in many cases are affected only to a limited extent by the internal administration of potassic iodide and mercury, unless, indeed, these drugs are pushed to a dangerous extent.

In the course of time the dog begins to arch his back, and the

motion of the hind legs becomes stiff and the gait straddling. By these signs the attention of the practitioner is directed to the inside of the thigh, and he there detects a *bubo* identical in its characters with that seen in man, and, like it, ultimately associated with chronic ulceration of the skin covering it. If these lesions are not syphilitic, what are they? I leave those who deny that the disease exists in the lower animals to answer the question.

In reference to the treatment of those important lesions, *bubo* and *chancre*, I may observe that the former is much more easily dealt with than the latter. In my experience the free inunction of iodine ointment, with the application of nitrate of silver to ulcerated surfaces when such are present, quickly produces resolution, but in obstinate cases I should advise iodine irrigation or extirpation of the tumor; no harm could result from the adoption of the latter course, and seeing that there is great danger of the gland becoming a centre of infection to the system it would be the wisest course to adopt.

In the treatment of chancrous ulceration of the penis we have an infallible remedy, if I be permitted to so designate it, in castration. I am perfectly well aware that some of my scientific friends will utter an exclamation of surprise and horror when this statement meets their eyes, but one fact is worth a dozen theories, and however unscientific and unsurgical the operation may at first sight appear, it is all the same an absolutely effectual and perfectly safe cure. In the case of valuable stud dogs the removal of the testicles would, I need scarcely say, be a matter of grave importance, as its adoption would mean annihilation of the procreative function; but in dealing with animals in which their use for stud purposes is only of secondary importance, the operation should unhesitatingly be performed if other remedies fail in effecting a cure. The first occasion on which I adopted the treatment in practice was in October, 1873, at which time I had under my care a terrier of nondescript breed (much prized as a companion by his owner) suffering from venereal sores on the penis and on the skin of the abdomen and thighs. After giving the usual constitutional remedies, as potassic iodide and

mercury, and the usual local applications, and nitrate of silver and sulphate of copper, a lengthened trial, I determined upon trying the effects of castration. My reason for performing the operation was that I observed whenever I manipulated the animal chordee became very marked, and this was followed by extreme vascularity of the penis and particularly of the tissues involved in the ulcerative process. In my own mind I came to the conclusion that if priapism were prevented this periodical hyperæmia would be done away with and *rest* insured to the cells of the diseased parts.

The operation was performed, the effects of it surpassed my most sanguine expectations, and the dog was quickly discharged cured.

(*To be continued.*)

WHY PASTEUR'S VACCINE FAILS TO PREVENT HOG CHOLERA.*

By D. E. Salmon.

In my former communication on this subject was given an account of an experiment made by the Bureau of Animal Industry with Pasteur's vaccine from which we concluded, some time in advance of the termination of the Nebraska experiment, that this vaccine could not be used to prevent hog cholera in the United States. In the first place, whether this vaccine is reliable when it leaves the laboratory of the great French chemist, or not, it certainly was not in a condition to give reliable results when it reached us, although it had probably not been prepared longer than three weeks when it was used. The first vaccine was of about the strength which we expected it would have, but the second vaccine was weaker than the first, instead of being stronger, as it should have been. This fact we do not understand, and it seems to us that, as both were subjected to the same journey, we are justified in entertaining the suspicion that they did not have the proper relative conditions of vigor when they left France. At all events, the vaccination was a failure, and the vaccinated hogs

* Reprint from the *Breeders' Gazette*, May 20, 27, June 3, 10, 1886.

contracted hog cholera, when exposed to it, just as readily and just as severely as others who had not been operated upon.

The great question of interest, and the one which it is the object of these papers to consider, is: Why does Pasteur's vaccine fail to prevent hog cholera? Dr. Liautard evidently believes that it is owing to the time required to bring the vaccine from Paris to America. Considering, however, the activity of the first vaccine, which evidently multiplied in the body of one of the vaccinated animals, since the germs were found in its internal organs at the time of its death, four days after vaccination, and considering also that two of the pigs were given at least twelve times the prescribed dose, it would appear that at least some degree of immunity should have been shown by these animals.

Dr. Liautard's explanation does not appear to the writer to be any nearer correct than that of Dr. Gerth; indeed, unless I am greatly mistaken, both of these gentlemen are radically wrong in their conclusions that the French disease, called by Pasteur *rouget*, is identical with the American disease which we call hog cholera. I explained this matter quite fully at the meeting of veterinarians and members of the State Sanitary Commissions held in Chicago last November; and Dr. Gerth evidently had my view in mind when he stated so positively that the germ in the Pasteur vaccine was identical with that found in hogs affected with hog cholera in Nebraska. The ridiculousness of such an assertion, when we come to consider the investigations that were made, would be amusing if it were not for the serious consequences which follow from deceiving the public with such rash and unreliable statements.

To understand the contested questions in relation to the germs of the swine diseases in France and America, it is necessary to refer briefly to recent European investigations. Pasteur's first paper on *rouget* appeared in 1882, and he then described the germ as having the form of a figure eight and resembling the fowl cholera microbe. In 1883 he confirmed these statements. From these papers we and many others were led to believe that he had discovered the germ of this disease to be one of the spherical, globular, or bispherical species of bacteria; that is, a micrococ-

cus. He even went so far as to admit that the bispherical microbe described by Dr. Detmers in the *American Naturalist* was identical with his germ. After this we were, of course, very much surprised to learn from Europe, through the correspondence of Dr. Sternberg, who had just visited Pasteur's laboratory, that Pasteur was sending out an elongated, rod-shaped organism, in other words a bacillus, and that he had apparently discarded the micrococcus which was first described. Later we received the work of Lydtin and Schottelius, giving full details of their very thorough investigations of Pasteur's vaccine and of the German disease called *rothlauf*, which is believed to be identical with *rouget*. They not only described, but figured the germ and demonstrated it to be a peculiar bacillus to which Pasteur's description of the germ originally found does not apply.

The vaccine which Dr. Liautard obtained for us from Pasteur's laboratory contained these delicate bacilli which were figured by Lydtin and Schottelius, and which had also been described by Lœffler and Schutz as causing the *rothlauf* of German swine. This microbe was very easily demonstrated by us not only in the vaccine liquid, but also in the blood and internal organs of the mice and the pig which died after inoculation with it. There is consequently no opportunity left at this time for any one to contest the fact that the active principle in Pasteur's vaccine is an elongated filament or bacillus, which is so distinct in its shape that it could not be mistaken for a micrococcus.

This microbe, which is probably the cause of a more or less serious and fatal disease of swine in Europe, is very delicate and difficult to see even under the best microscopes unless the preparations are suitably stained. The vaccines contain a relatively small number of these germs, and unless mounted and stained they are very easily overlooked. The liquids and tissues of animals which have died from this contagion contain the germs in still smaller number, and to identify them without staining is next to impossible.

Now, the question is, how were the microscopical investigations made in Nebraska? It is very well known that no one connected with the investigations there had previously been engaged

in the study of disease germs, and consequently it is very certain that the material was not at hand to make a satisfactory investigation even if the microscope was as perfect a one as can be made. I have taken some trouble to collect information in regard to this matter; and from various sources I learn—first, that no stained preparations were made for examination; secondly, that the liquids examined were gathered from the dead animals without any precautions for excluding atmospheric germs; thirdly, that none of the germs were cultivated for study; fourthly, that the bacillus, which is the essential element of Pasteur's vaccine, was not seen at all, but was entirely overlooked, and a contaminating germ of very different appearance—in other words a micrococcus—was considered to be the Pasteur germ, and the conclusions were drawn from this. The germs found in the hogs sick in Nebraska were also micrococci, and consequently could not be the same as those which Pasteur believes to be the cause of the swine disease of France.

Having been engaged in the microscopical study of swine plague for the past eight years, I think I can say, without laying myself open to the charge of egotism, that I know something about the germs which exist in the bodies of sick hogs, and also something about the proper methods of investigation and the difficulties in the way of their practical application. From this knowledge I have little hesitation in concluding that if the germ found in the affected hogs in Nebraska was a micrococcus, as those who saw it believed and as I have every reason to believe, then Dr. Gerth neither saw Pasteur's germ in the vaccine nor the true germ of hog cholera in the specimens examined from the diseased hogs. I see no reason to doubt, therefore, that the Nebraska experiments were a failure from every point of view; the vaccine was too old to give results of any value, and the microscopical examination failed to reveal the pathogenic germs.

This result of such incomplete and brief researches is not to be wondered at; indeed, it would have been next to a miracle for any one to make sufficiently brilliant discoveries to settle such difficult and contested questions in so short a time, with so little work and with so few facilities. The remarkable part of

it is that Dr. Gerth should come before an intelligent public and make these preposterous claims with such material as he had to base them upon.

The references to Prof. Bessey in the report under consideration seem to have been made without his desire or consent. This gentleman is a scientist whose attainments and opinions are worthy of great respect; but he is a botanist, has never been engaged in swine-plague investigation, and makes no pretensions to being an authority on animal diseases. The material in this case was taken to him because he was the possessor of a good microscope. He placed it under the instrument for examination, and when he says that the germs revealed were micrococci I have confidence that his statement is correct.

That the general student of botany, or even of bacteriology, should be puzzled to know what particular germ is the cause of American swine plague is not surprising. The whole science of disease germs is new, and is passing through a transition period. Two widely different germs have been lately described in Germany as producing contagious swine diseases—one of these, the germ of *rothlauf*, is identical with Pasteur's bacillus; the other is entirely distinct, and is said to produce *schweineseuche* (swine plague). In France two or three different germs have been described, but it is not yet certain whether more than one disease has been studied in these investigations. In the United States a number of distinct germs have also been referred to by investigators as the cause of American swine plague. These different opinions have led to much confusion, and no one but those who have been continually at work on this question can have an intelligent opinion of its present condition. Probably more hard work has been done in regard to this disease, and more experiments have been made by the Bureau of Animal Industry at Washington than anywhere else in the world, and we expect to settle the contested questions in a short time by the presentation of evidence of the most convincing character.

In the May number of the AMERICAN VETERINARY REVIEW Dr. Liautard makes some additional remarks in regard to hog cholera which we find it convenient to take as texts for the different

points discussed in this communication. He says; "It *has been thoroughly* and *without doubt* demonstrated that inoculation is the *only* reliable measure of prevention. It is true that before inoculation can be introduced into this country we must have a definite and satisfactory answer to an important question furnished from one source or another, aud must settle the query, 'what is hog cholera'—is it the same disease here with that which prevails in Europe? Veterinary authorities on this side of the Atlantic seem to disagree on this important point." Are these not rather contradictory sentences to occur side by side in the same paragraph? If veterinaiy authorities are not agreed as to whether *rouget* and hog cholera are the same disease there must be very good reason to doubt if Pasteur's investigations apply at all to hog cholera. This being the case, there has been nothing whatever *demonstrated* as to the value of inoculation as a means of preventing hog cholera; in fact the only experiments made with Pasteur's vaccine have signally failed, and we are, consequently, very much surprised that one so able and so conservative in his views as Dr. Liautard should write that inoculation had been demonstrated *thoroughly and without doubt*, not only to be a reliable measure of prevention, but *the only reliable measure.* We fear that the Doctor's enthusiastic belief in the measures proposed by his great fellow-countryman has led him to make somewhat exaggerated claims in regard to his measures—claims which the "cold logic of facts," as Dr. Gerth puts it, will hardly sustain.

This naturally brings us to a consideration of the differences between the swine diseases of Europe, which Dr. Liautard unaccountably refers to as one disease, and between the diseases of Europe and the one contagious disease which has been described as affecting American swine. To give a clearer idea of his views I quote: "Is the *rouget* of France, the *schweineseuche* of Germany, and the pneumo-enteritis of England and the cholera of America one disease? If we are to accept what has been written by competent authors in all these various countries there does not seem to be room for a doubt. The symptoms and the lesions of which we read descriptions by French, German, Eng-

lish and American authors are almost literally the same. The reports of those who have seen it in these various countries contain no essential differences in their accounts of the different nationalities of the different breeds. Notwithstanding this, however, statements of the opposite opinion have been made public, in a number of instances, as of course there will always be many minds among many men. We who have seen it in both France and America, and some of our friends who have observed it in Germany and the United States also, are in no doubt respecting their similarity, and are so strong in our consideration that we cannot see any other way to recommend in the prophylaxy of this scourage of swine than inoculation."

Recognizing the fact that this paragraph is aimed at the writer, as the only one who has denied the identity of the *rouget* of France and the hog cholera of America, the challenge implied in these lines is unhesitatingly accepted, and an attempt will be made to show either that Dr. Liautard and the gentlemen who share his view are not well informed in regard to the recent European literature on this subject, or that they have been able to obtain information which is not accessible to the writer. Should they decide that the latter alternative is the correct one we shall be happy to have them present their evidence.

In the first place it is well to remember that French veterinarians have described two or more epizootic diseases as affecting pigs, and a very slight examination of their literature shows that even now there is the greatest confusion in regard to the whole subject. Bénion, who is the author of the only complete scientific treatise on hog diseases that has appeared in recent years in France, classes *rouget* and *rougeole* together and calls it a skin disease; he also describes a different disease, which he calls charbon, but as recent investigations seem to demonstrate that the hog is not subject to charbon, this second disease is of an undetermined nature. Zundel, whose knowledge of this subject will hardly be questioned, treats of *rouget* under the heading of epizootic erysipelas of the pig, and says that *rougeole* is easily distinguished from it; he also refers to still another disease which he calls charbon. Cornevin, in his work on *rouget*, finds it neces-

sary to admit a second disease and calls it *septicemie gangreneuse*. Dr. Paul Cagny, who was sent by Pasteur to carry out the vaccinations in Baden, says in a recent letter to Dr. Cornil: "It is necessary in the first place to indicate that two different bacterial (*microbiennes*) diseases have been confounded—one is certainly the *rouget* caused by the short and fine microbe described by Pasteur and Thuillier and for which the preventive vaccination should be employed; the other is caused by a large and long microbe discovered by Klein, of London." In Germany there is the *rothlauf*, now admitted to be identical with *rouget*, but it is still an open question if the *schweineseuche* is the same disease. Consequently, if there are two or more different diseases in Europe they cannot all be identical with our hog cholera; and if it should happen that we have confounded two or more diseases in this country, then Pasteur's vaccine could only prevent a part of our losses, admitting that we have a disease identical with *rouget*.

As so little is settled about the swine diseases of Europe we find it necessary in any comparisons of value to take the characters of *rouget*, as the malady is produced by the Pasteur virus, or by contagion from a disease recognized by competent authority to be the same. We may compare the characters of that disease with our hog cholera under the following heads:

1st. *Incubation.*—The period of incubation—that is the period which elapses between exposure to a contagion and the appearance of the first symptoms of the malady—is certainly one of the most important characteristics of a contagious disease. With our hog cholera this period varies from five to eighteen days in summer and from ten to twenty-one days in winter; the average is seven or eight days in summer and two weeks in winter. This refers to cases of disease produced by exposure to infected pens or to sick hogs. The symptoms appear somewhat sooner when animals are inoculated, but the period is even then very rarely as short as four days, and is generally twice this length. Now taking the experiments of Cagny in France (*Bul. Soc. Cent. de Med. Vet.*, 1885, p. 151) and Lydtin in Germany (*Die Rothlauf der Schweine*) we find that when pigs are exposed to the contag-

ion of *rouget* they sicken in from one to six days, the average being from two to four days. When inoculated some were sick the same day; the average incubation was, according to Cagny, less than twenty-four hours, and according to Lydtin less than forty-eight hours. The period of incubation consequently differs remarkably in the European disease from what is seen in our hog cholera.

2d. *Duration of Disease.*—Animals affected with hog cholera are usually sick from one to two weeks before they die. Occasionally the plague is more rapid in its course and death occurs within three or four days, but the average period of sickness is not less than eight or ten days. With *rouget*, according to Lydtin, the animals are only sick from thirty-six to sixty hours, or an average of about two days. Here again we see a most remarkable difference between the two diseases.

As this letter is already long I will take up other points of difference in another communication.

(*To be continued.*)

AMERICAN VETERINARY COLLEGE.

HOSPITAL RECORDS.

By R. Weir, D.V.S., House Surgeon.

ADENITIS AND ITS RESULTS.

On Sunday morning, April 11th, 1886, there came under my observation a chestnut gelding, eight years of age, fifteen hands two inches high, that had been discharged about ten days ago, as convalescing from an attack of pleuro-pneumonia.

The symptoms now exhibited were noisy breathing, discharge of a purulent character from both nostrils, swollen and painful intermaxillary glands, temperature 104 2-5 degrees F., respirations and pulse almost normal in number, and almost complete loss of appetite: upon which a diagnosis of adenitis was made, and the following treatment given: Fomentations and hot poultices to the enlarged glands, fumigations of steam; all to be continued until the abscesses were all opened. Administered internally stimulants three times a day.

After a few days the breathing was normal, pulse and respirations were about the same, and the temperature varied from 102 to 104 2-5 degrees until April 26th, when it rose to 106 2-5 degrees, pulse 68, respirations 32, and the appetite not improving. He was now given quinine twice a day, stimulants, tonics, milk, hay tea, gruel, etc.

During all this time four abscesses in the intermaxillary space were opened and had again healed, yet the condition of the animal was not improving, and on examination dullness wus found over the lungs, and it was thought that abscesses were forming in the lungs.

The first of May found him with a temperature of 101 degrees, respirations 24, pulse 48, and purging freely, for which opium was given with success in ehecking it.

The weakness now increased very much; nutrition was given in drench and by rectum, but the weakness continued until May 9th, when he died.

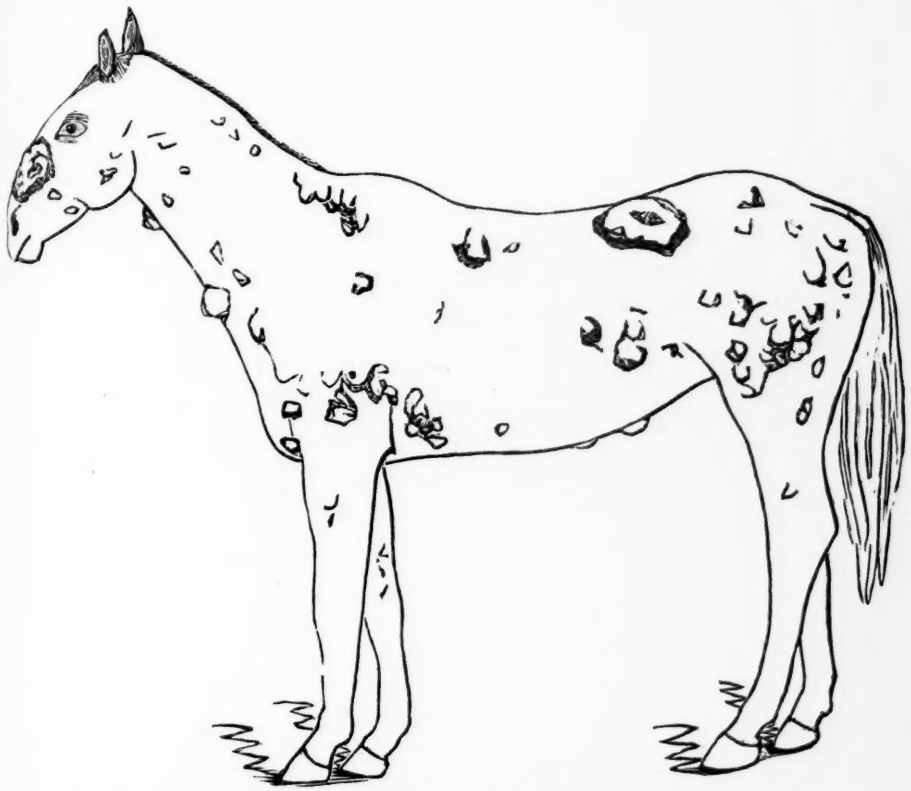
The post-mortem examination held the same day showed: Small enlargements in the intermaxillary space, enlarged prepectoral glands, pleurisy with effusion, old costal and pulmonary adhesions, and lymph patches on both; diaphragmatic pleura was highly inflamed; right lung edematous and of recent occurrence; left lung showed a portion at the base that was nearly normal; the apex showed pneumonia, and in the middle portion was a large abscess occupying about one-fourth of the lung; heart normal except in the right side was found an ante-mortem clot that extended into lungs; abdominal cavity contained a large quantity of fluid; beginning of peritonitis and mostly of the small intestines; the kidneys slightly congested; spleen highly congested; the liver of a nutmeg appearance and containing two large abscesses from which escaped laudable pus slightly greenish in color; and the mesenteric glands all greatly enlarged.

LEUCOCYTHEMIA OF THE SKIN.

By J. A. Walrath, D.V.S., House Surgeon.

Toward the latter end of the spring session a bay mare was brought to one of the free clinics covered with a great number of

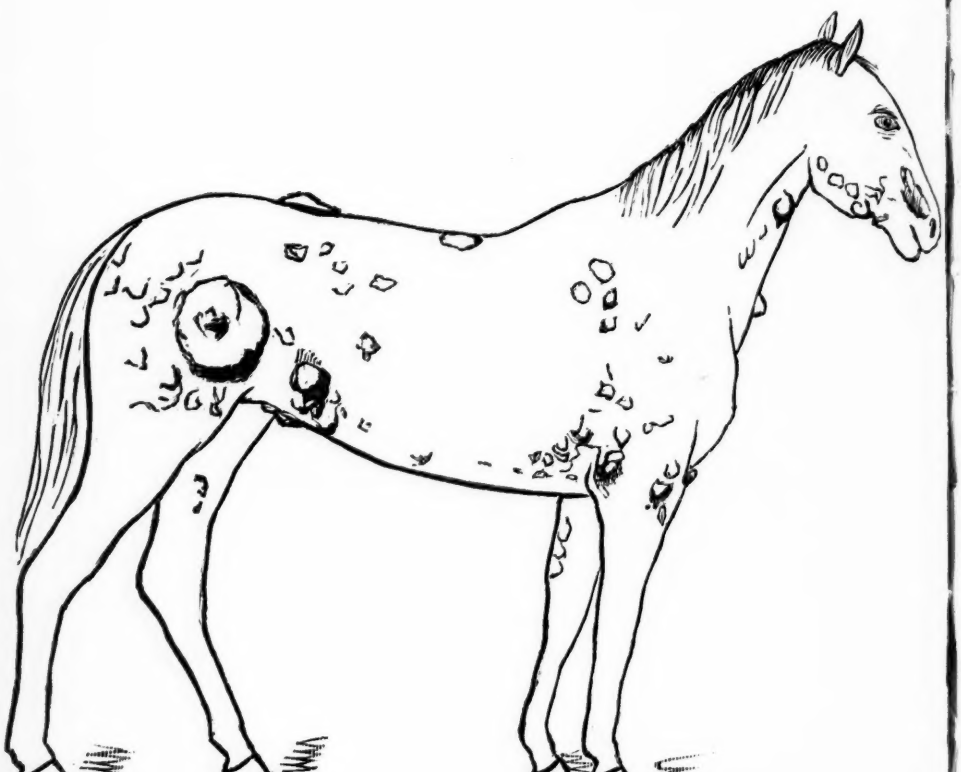
tumors, varying in size from a walnut to a very large apple. It being a case of much interest and of comparatively little value as a working animal, an attempt was made to purchase her for clinical observation; the owner however, refusing the offer, she was lost sight of until May 28th, when she was once more brought



(Near Side View of a Mare with Gangliona Leucocythemia.)

back to the college, the owner now being only too glad to get rid of her, fearing arrest by the officers of the "Society for the Prevention of Cruelty to Animals," as one of the largest of these tumors, situated on the head in the right facial region, about three inches below the eye and extending downward to about an equal distance above the nostril, was denuded of skin and presented a raw granulating surface some five inches in length and

four in width, and her general appearance was such as to attract the attention and sympathy of passers by. The animal had great difficulty in breathing, and roared continually while moving, as some of these growths were located in the upper air passage, and so materially constricted these channels that suffocation was



(Off Side View of the Same.)

threatened at times when the animal's pace was hurried. Other enlargements were noticed over nearly every portion of the head, but principally on the sides of the face, on the parotid region, around the nostrils, and in the intermaxillary space. Here in this latter place they were so thick as to have the feeling of large marbles under the skin. Beginning in the jugular groove, they seemingly followed the direction of the lymphatic glands, as

clusters of them were scattered over the outside of the arm, on the chest, between the forward legs and in the axilla. Then, beginning opposite the point of the elbow and running backward on the body, covering the abdomen and flanks, as well as the inguinal region, and extending upward on each side of the perineum to the base of the tail, around which numbers of them were growing. A considerable number was also seen on the hips, on the inside and outside of the thighs. And two very large ones, situated, one on the right side in the triangular space comprised between the "external angle of the ilium" and femur, being round in shape and standing out from the skin an inch or more, blackish in color and devoid of hair over its outer face. The other one was on its left side, high upon the lumbar region, about a hand-breadth from the vertebral column, and extending from the last rib backward about eight inches; this one was rather oblong in shape, but had the same general peculiarities as the one on the opposite side. These swellings were hard, not painful to the touch, and seemed to be in the connective tissue under the skin, as they could easily be displaced by manipulation.

The total number of these growths visible when she entered the hospital was about one hundred and fifty. Her temperature and pulse were normal, the appetite good, coat sleek, and apparently in good health, and suffering no inconvenience from them except in respiration. After her admission she was placed under observation, and was exhibited to many veterinarians and physicians. One prominent among the latter was asked to examine the blood under the microscope, to determine if the relative proportion of white to red cells was increased. After drawing some blood and making a hasty examination, he concluded that the white cells were not in excess, and that the blood, as far as he had gone, was normal.

The appearance presented by the blood when allowed to stand a few hours in a closed test tube, was somewhat as follows: It separated into a clot of a very deep bluish purple, over which was seen the serum of a light straw color, very thin in consistency and transparent. Each of these different portions of the blood occupied about an equal space. In the centre of the serum

another clot of a faint red hue extended upward from the purple to within about an inch of the top of the fluid; this last clot was cylindrical in shape and surrounded on all sides by the liquid serum.

Not being satisfied with the results obtained from the first examination, measures were taken for a more complete and careful comparison of the vital fluid. With this in view, a small quantity was taken to Dr. T. M. Prudden, "Director of the Pathological Laboratory of the Alumni Association of the College of Physicians and Surgeons," and on June 12th the following letter was received from that gentleman: "I have made 120 counts of the blood, and the average result is 1 white blood cell to 265 red."

The animal had at this time been here a little over two weeks; the swellings had been gradually increasing during the time; slow it is true, but still enough to convince the observer that they were developing both in size and numbers, as several were noticed now that did not exist when she was first admitted. The large one referred to as being on the right side, seemed hot to the touch, and when on the 13th the animal was destroyed for a post-mortem examination, the temperature of this one was taken by making an incision through its thickness and inserting a thermometer, which registered $101\frac{2}{5}°$, or $1\frac{2}{5}°$ higher than her bodily temperature had been since she had been under observation. This tumor when enucleated weighed four pounds, measured two feet in circumference, and was four inches thick. The other large one when taken out weighed three and one-half pounds, measured the same in circumference as the first one, and was three and one-half inches in thickness.

The septum nasi was also the seat of two of these growths; one located well forward near the entrance to the nasal cavities, the other farther back, directly in the middle of the septum, projecting on either side of it to quite a distance, which readily explained the cause of roaring that was so marked during life. It was also hoped on post-mortem to be able to still further confirm the diagnosis that had been made previous to the examination of the blood by some important lesions of certain of the external organs, but after a most diligent search no pathological changes

were observed in any of them. Even the spleen, which is said to be always affected in this trouble, was apparently perfectly healthy and showed not the slightest trace of disease. Notwithstanding the negative result obtained from the post-mortem, the condition of the external manipulations with the actual count of the blood cells are sufficient to establish the diagnosis.

EXTRACTS FROM FOREIGN JOURNALS.

OBLITERATION OF THE POSTERIOR AORTA IN A HORSE.

By M. Pirl.

This is a case of obliteration by stenosis, of the posterior aorta, with the continuation of the circulation by collateral branches, in a pony fourteen years old, which died almost suddenly after a sickness of a longer period of time than usual, and at the post-mortem of which unexpected lesions were found. The heart was considerably hypertrophied, the left ventricle especially, the walls of which were four and a half centimeters in thickness. The cardiac envelopes and the valves remained healthy. The posterior aorta measured thirteen centimeters in circumference, and was one-half centimeter thick in its walls. These dimensions existed in a length of twenty centimeters from the heart, the vessel then becoming more and more retracted, until it represented a thin cord of the size of a small finger, upon a length of five centimeters. This cord throughout was entirely solid, not allowing the introduction of the finest bougies. The arterial wall was weak and extraordinarily thin. Further on, the aorta was dilated, and its walls increased in thickness. Beyond this, two aneurisms were found containing parasites, surrounded by clots of blood adherent to the vascular walls, which were partly calcareous. Towards the end of the aorta were numerous transversal abrasions of the internal coat, leaving the middle layer unprotected and exposed. Several vessels, of the dimension of the carotid, escaped from the aorta in front of its contracted part, and carried the blood to the thoracic and abdominal organs, following the course of the artery they replaced. The intercostal arteries presented numerous irregularities, and the internal thoracic, largely developed, offered in

several places sacciform dilatations. A similar disposition existed upon all the arteries rising from the anterior aorta, as well as in the dorsal branches of the intercostals. On a level with the tenth dorsal vertebra there was an artery as large as a finger.

Taking into consideration the various lesions observed, the author believes that all the alterations originated from the lesions of the posterior aorta. The retracted portion of the vessel probably corresponded with an aneurism which was cured by slow and progressive obliterations; the diminution of permeability, and afterward the complete obliteration of the aorta, had given rise to a constantly increasing obstruction of the circulation; the arterial pressure had increased forwards, and had been followed by the hypertrophy of the left ventricle, as well as the dilatation of the vessels directly under its control, and consequently, the formation of a collateral circulation. This supplemental circulation by anastomotic breach may be of no great importance, where the obliterated vessel is small, but when the artery is such as the aorta itself, it becomes an exceptional phenomenon.—*Annals de Belgique.*

ACTINOMYCOSIS OF THE SPERMATIC CORD OF THE HORSE.

By M. Johne.

As far back as 1819, Rivolta and Micellone had observed the presence of a micromycete in the tumors of the spermatic cord of the horse. This parasite presented itself under the appearance of small granular masses, whose structure greatly resembled the radiated disposition of the bovine actinomycosis. Its constituting elements, however, were much simpler, consisting only of large granular cells, the threads and granulations in mass being absent. For this reason, Rivolta classified this parasite in a special gender which he named *discomyces equi.*

Subsequently, Professor Johne, who ignored the observations of Rivolta, observed the presence of actinomycosis in several cases of the spermatic cord. He designated this form of the affection under the name of "*chronic mycotic funiculitis*, or *actinomycotic.*"

Out of thirteen cases of funiculitis operated for by Siedamgrotzky and by Johne, four belonged to this category.

Two other observations of the same kind are recorded. One is recorded by Siedamgrotzky; but in this case, the parasite was represented only by the raspberry form, or grouped masses, whose rounded elements formed in gathering together, spherical lobules, resembling very much small acini glands. Johne considers these to be a peculiar micrococcis, having no relation to the bovine actinomycosis; but he attributes to it a pathogenic action in the production of funiculitis.

The other observation is from Mr. Semmer of Dorpat. In an animal killed for anatomical purposes, he found purulent collections in the middle of old inflammatory lesions of a testicular cord, of the inguinal glands of the same side, on the surface of the liver, and of the diaphragm, and on various parts of the peritoneum. There were some also in the lungs. The pus of these abscesses contained small brown-yellowish masses, whose microscopic aspect much resembled, according to Semmer, the structure of actinomycosis.

Amongst the cases of Johne, one was entirely conclusive; the parasite resembled exactly that found in the maxillary tumors of the ox. Two others were nearly as positive; the fourth was less so.

According to the author, these are simply a degraded form of actinomycosis bovis, a form which he has often seen in cattle, and especially in a case of a tumor of the recticulum.

In the microscopical point of view, the characters of the alterations of the ventricular cord resemble also those of the actinomycoma of the lower jaw of cattle. Contrary to what exists in ordinary cases of scirrhous cord, in the parasitic form, it is the indurated tissue which predominates; the fistula and the purulent cavities are always in small number, and are often absent.

If a section is made in one of these tumors, its fibrous bottom appears lardaceous, and shows small nodosities, yellow or reddish, isolated or collected together, and varying in size from that of a pea to that of a hazel nut. In these nodules, as in the purulent collections, when they exist, a fine yellowish powder appears, resembling sand or pulverized sulphur. Under the microscope these granulations are seen to be made of tufts of actinomycosis, more or less modified.

It becomes evident from these observations that certain tumors of the cord following castration may be due to pathogenic germs.

According to Johne, these would rise from the bedding of the animals operated upon.—*Annales de Belgique.*

THE MICROBE OF RABIES.

By M. Rivolta.

The method by which the author is said to have demonstrated the constant presence of a spinal micro-organism in rabies, is thus described:

After hardening the spinal marrow, the medulla oblongata and other organs of animals which have died with rabies, in alcohol, he makes fine sections, which he places in chloroform for twenty-four hours, in order to clean them from the fat they may contain. These are again placed in alcohol, and subsequently in a mixture of 10 parts of an aqueous solution of caustic potash (10%), 3 parts of distilled water, and 3 of glycerine, where they are left for a period of five or six hours at most.

The sections are then put into a solution of blue of methyline, where they remain for a few minutes, or until they become colored. They are then washed with distilled water, and spread on a slide and slowly dried in a moderate heat.

This preparation is then sealed with Canada balsam dissolved in chloroform. If the slide is examined at that moment it presents a diffused coloration, but it is easy to bring out the peculiar coloration special only to the bacteria. By carefully exposing the slide to the flames of an alcoholic lamp, until the Canada balsam begins to boil, and then leaving it to become cool, the tissue loses its color, but the bacteria remains intact. By repeating this process two or three times, the bacteria are brought out in a handsome blue color, projected upon a colorless ground.

Thus colored, the bacteria is seen to be formed of several round or oval granulations, united in chains, though at times, in some few cases, they seem uniform and appear to be formed of one single body.

They are found in great numbers in the medula; they are also numerous in the marrow, but less so in the cerebral hemispheres;

they are also seen in in the epithelium of the parotid; they are rare in the spleen and the kidneys, but numerous in the liver, in the vicinity of the hepatic cells.

Professor Rivolta diluted in distilled and sterilized water the bulb of a rabid rabbit, and filtered the produce through a china shifter. With the liquid thus obtained he inoculated three rabbits by trephining; at the same time, and by the same process, he inoculated five other rabbits with the liquid remaining on the shifter. These last two animals died of rabies after eighteen days,while the first three continued free from disease up to four months after the inoculation.

This discovery needs confirmation. It is, however, a very important matter, and may contribute to the illustration of the pathology of this frightful disease.—*Annales de Belgique.*

CASTRATION OF CRYPTORCHID HORSES.

By C. E. Munn, Veterinary Surgeon, Watertown, Dakota Territory,

I have been much interested at different times in articles published on this subject in varions works on the science of veterinary surgery. There have been several methods described and recommended. I have not as yet, however, seen the so-called Farmer Miles' method published; and, as it is most in vogue in this country with those veterinary practitioners who are successful in performing this operation, it must surely claim the attention of all veterinary surgeons who may be called upon to castrate a cryptorchid horse.

It will not be necessary to enter into any of the preliminaries attending the operation, Mr. L. Nielsen, in his excellent article on this subject, in the January and February numbers of the *Veterinary Journal*, having given full particulars as to the proper condition of the animal at the time of the operation, also most desirable age, and so on.

The operator should be very particular in securing the subject to be operated upon. The main points are, to have the animal securely bound, so that there will be as little struggling as possible, the posterior extremities well flexed, the fetlock joints

in close proximity to the stifles. Farmer Miles' method of securing, as shown in Fleming's "Operative Surgery," Vol. I., pages 36-40, is one of the best and safest that I have seen in practice. The subject having been thrown, and properly secured, the operation is performed as follows:—

The parts should be thoroughly cleansed with a warm antiseptic wash, all instruments having been previously disinfected with carbolised oil, or kept within reach of the operator in a basin of carbolised water. The incision should be from 10 to 13 cm. in length, commencing posteriorly, just above the anterior border of the pubis, and a little farther from the median line than is customary in castrating naturally-formed horses. The incision is to be made through the skin only. The fingers of one hand should be inserted beneath the skin, and worked a little to the outside and back, breaking down the subcutaneous tissue until Poupart's ligament is reached. This will be but a short distance. Then the index and median fingers are to be passed over the posterior border of the ligament into the abdominal cavity, care being taken not to injure the ligament or to make the opening into the cavity any larger than necessary. The vicinity of the internal inguinal ring and brim of the pubis can then be explored, and, in most cases, by a careful rotary motion of the fingers, either the gubernaculum testis, the spermatic cord, or the testicle itself can be secured, and the testicle brought to the external opening. If the testicle or either of the guides to it are out of reach of the fingers, the whole hand will have to be inserted. It is very evident, therefore, that the operator with a narrow thin hand has considerable advantage over one whose hand is large and thick. The testicle having been brought to the external opening, I consider that the best method of removing it is by the écraseur, but the instrument must be a good one, that will operate without hemorrhage. Such écraseurs are not common—at least, such has been my experience.

If the operation is properly performed, and the animal well cared for afterwards, there need be but little fear of any bad results. The animal should be kept in a dry, well-ventilated stall, and given small feeds of easily-digested food, and but a

small amount of water. There is generally more or less fever for four or five days; after that time, if the animal is doing well, he should be given a little exercise daily, but not until the fever has subsided. The average time for recovery is about three weeks.

Carbolized oil is freely used by some operators on their hands, acting as an antiseptic, and also facilitating and rendering less irritating the passage of the hand from the external opening to the posterior border of Poupart's ligament into the abdominal cavity.

Anæsthetics are hardly ever used in this country during the performance of this operation.

This method of castrating cryptorchid horses has proved very successful in this country, when skilfully performed. The few deaths that I have known have occurred from peritonitis, supposed to have been caused through carelessness of the operator in not observing proper antiseptic precautions.

VETERINARY LEGISLATION.

CHAPTER 587.

AN ACT IN REGARD TO THE AMERICAN VETERINARY COLLEGE, PASSED JUNE 5TH, 1886; THREE-FIFTHS BEING PRESENT.

The people of the State of New York, represented in Senate and Assembly, do enact as follows:

SECTION 1. The American Veterinary College, organized under the provisions of chapter three hundred and nineteen of the laws of eighteen hundred and forty-eight, entitled "An Act for the incorporation of benevolent, charitable, scientific and missionary societies," and the acts amendatory thereof, by certificate dated April twenty-seventh, eighteen hundred and seventy-five, and approved by one of the justices of the Supreme Court, and filed in the office of the clerk of the county of New York, the twenty-ninth day of April, eighteen hundred and seventy-five, is hereby declared to be legally incorporated for the purposes of maintaining and conducting a veterinary medical college and a hospital devoted to the education of men in the several departments of the medical sciences necessary to qualify them for the practice of veterinary

medicine and surgery, and of conferring the degree of Doctor of Veterinary Surgery.

SEC. 2. All degrees heretofore conferred by said College are declared valid.

SEC. 3. Said College shall be subject to the same duties, obligations and liabilities and to the same control and visitations of the regents of the university as colleges and universities chartered by said regents.

SEC. 4. This act shall take effect immediately.

STATE OF NEW YORK,
OFFICE OF THE SECRETARY OF STATE.

I have compared the preceding with the original law on file in this office, and do hereby certify that the same is a correct transcript therefrom, and of the whole of said original law.

Given under my hand and the seal of office of the Secretary of State, at the city of Albany, this 12th day of June, in the year one thousand eight hundred and eighty-six.

FREDERICK COOK, *Secretary of State.*

REVIEW.

LE CHEVAL, Exterieur Regions, Pied, &c., &c. (THE HORSE, External Regions, Foot, &c. &c.), with 16 Plates. By E. CUYER, Professor of Anatomy, and E. ALIX, V.S.

W. R. Jenkins has received this magnificent and excellent work, and has it for sale amongst his veterinary works. Though written in French, it will prove a great acquisition to those who are paying special attention to the subject, and will prove of great and essential value, especially on account of the atlas which accompanies the book. This consists of 16 plates, exhibiting the anatomy of the animal in cuts superposed in the various layers of muscles as they exist in the living animal, and as the student will study them on the table of the dissecting room. These plates are minutely correct, and must effectually assist the ordinary practitioner in refreshing his memory, as they must also facilitate the study of the new beginner. These plates, we understand, can be obtained separately, and the French nomenclature of parts can be readily overcome by reference to the classical works of anatomy now in publication.

SECOND ANNUAL REPORT OF THE BUREAU OF ANIMAL INDUSTRY. 1885.

This large volume of over 600 pages is before us, through the kindness of the principal collaborator, Dr. E. Salmon—principal collaborator in truth, since mostly one-half of the work is the fruit of his labors. His reports embrace important investigations of contagious pleuro-pneumonia, swine plague, southern cattle fever, gapes in fowls, verminous bronchitis, and meat cattle quarantine. The latter half of the work contains the reports of agents and inspectors of the Bureau, upon some interesting agricultural subjects. The last 100 pages give a synopsis of the enormous amount of work performed by the veterinarians of the Bureau, in their inspections respecting the existence of pleuro-pneumonia in New York, New Jersey, Delaware, the District of Columbia, Maryland, Virginia and Kentucky. A careful examination of this portion of the book proves that the gentlemen engaged in the inspection have done well, and that their positions have been far from being sinecures.

To veterinarians, however, we believe the first part of the report will prove most interesting, and an attentive reading of the various articles cannot fail to gratify the reader. But amongst them all, probably none will deserve more attention than that which relates to swine plague, a better name, no doubt, than that of hog cholera—if, as Dr. Salmon almost proves, the disease which prevails here is not, after all, that which many have been accustomed to consider the same as the disease of Europe, known by the same name. Much had been done by the chief veterinarian of the bureau; for years he has worked in the investigation of the scourges of swine, and no one in this country has had better opportunities for studying not only the symptoms of the lesions, but everything connected with that disease. And though some of the ideas he advanced some years ago may be surrendered to make room for others of a different nature, there is no doubt in our mind that all that Dr. Salmon has said and written in the second Report is done with conviction and with evidences to back him. He has stated that the swine plague of the United States is not the same as that of Europe, and when he has been convinced he has said; "Do not believe in Pasteur's

vaccine—it cannot do us any good. It may prove the means of introducing a new disease which will bring untold loss to the swine breeders of this country."

The manner in which he has conducted his investigation, the careful inoculations, the attentive manner in which the post-mortems have been made, the description of the new microbe to which the disease is attributed, all in fact, are minutely described in the Report; and the handsome plates that illustrate it assist considerably in the study of the whole subject. The conclusions of the Report are very important, and give in a few words material for careful consideration. They say:

"The preceding investigations definitely settle certain controverted points concerning the etiology of swine plague, which may be briefly summarized:

"1st. Swine plague is caused by a specific microbe multiplying in the body of the diseased animal. The microbe probably belongs to the genus bacterium, and has the power of spontaneous movement. It is easily cultivated in nutritive liquids, but grows less readily on gelatine, which it does not liquefy.

"2d. When introduced beneath the skin, this bacterium is fatal to pigs, rabbits, guinea pigs, mice, and a certain percentage of pigeons. It is also fatal to pigs when introduced with the food, or when they feed on the internal organs of swine which have died of the disease.

"3d. The disease described in France as *rouget*, in Germany as *rothlauf*, and for which Pasteur has prepared a vaccine, is caused by an entirely different microbe. The vaccine for this disease does not protect against swine plague.

"4th. The introduction of Pasteur's vaccine is not only useless, but may contribute to the introduction and spread of a disease, the existence of which in this country has not yet been demonstrated."

THIRTEENTH ANNUAL REPORT OF THE NEW JERSEY BOARD OF AGRICULTURE.

Through the kindness of Dr. W. Herbert Lowe we have received this little volume, which amongst the interesting subjects it contains, gives a good account of the work done by the various

veterinary inspectors of the State. Pleuro-pneumonia, hog cholera, tuberculosis, rabies, glanders, have prevailed more or less in the little State, and have carried away quite a large number of animals.

NOTICE.

The Secretary of the United States Veterinary Medical Association earnestly requests that all who have not received their certificates of membership to said Association to write him prior to the annual meeting in September, in order that certificates may be properly signed. They will be promptly sent by express to all who have paid the initiation fee.

CH. B. MICHENER, Sec'y,
269 West 38th Street, N. Y.

SOCIETY MEETINGS.

NEW YORK STATE VETERINARY SOCIETY.

The regular monthly meeting of the New York State Veterinary Society was held on Tuesday, June 8th, in the lecture room of the American Veterinary College, Dr. S. S. Field in the chair.

There being only a few members present, the calling of the roll and reading of the minutes of the last meeting were dispensed with.

Committee on Testimonials reported progress.

Dr. R. A. McLean gave notice of the following change of By-Laws: That Section 1 of Article I shall read after the word "on" "the second Tuesday of each December, March, June and September, at such place and time as may be determined upon by the Board of Censors; such Board notifying the Secretary, who shall in accordance therewith call such quarterly meeting as hereinbefore provided for."

On motion, Drs. Pendry and McLean were appointed a committee to procure a suitable seal for the Society.

A telegram was read from Dr. R. W. Finlay stating that he could not be present.

The Secretary read the following letter from Senator Fagan, which was ordered to be entered on the minutes:

SENATE CHAMBER,
Dr. W. H. Pendry: ALBANY, May 19, 1886.

DEAR SIR.—Yours of the 17th inst. received. I am pleased to learn that my humble efforts on behalf of the New York State Veterinary Society are appreciated; such expressions are encouraging to every public officer. Hoping the measure, now that it has become a law, will inure to the advantage of the profession. I remain,

Yours respectfully, EDWARD F. FAGAN,

A letter was read from John J. Foy, V.S., stating that by reason of change of residence he desired not to become a member of the Society; and also one from Dr. R. Kay, asking to have his name struck from the roll of membership.

On motion, meeting adjourned till second Tuesday of September.

W. H. PENDRY, D.V.S., *Secretary.*

NEWS AND SUNDRIES.

A Bill to regulate the practice of veterinary medicine and surgery has recently been defeated in the Ohio House of Representatives.

Good Appointment.—Dr. Daniel Lemay, of Kansas City, has been appointed Veterinary Surgeon of the 1st U. S. Cavalry, and ordered to Fort Custer, Montana.—*Prairie Farmer.*

Pasteur Institute.—The subscriptions for the Pasteur Institute have reached 1,100,000 fs.; over 220,000 dollars. All classes of societies are subscribing to it. The number of persons treated to the end of May was 1,100.

Rabies in Paris.—The Council of Public Hygiene in Paris reports that in the year 1885, 518 animals have been affected with hydrophobia, viz: 503 dogs, 13 cats, 2 horses and 527 have been reported as suspects. Of 64 persons officially recorded as having been bitten, 19 have died with rabies.

A New Veterinary School.—The President of the Republic of Guatemala has issued a decree establishing a veterinary school in the capital of the republic. A French veterinarian, Mr. Soula, has been appointed director of the new institution, which is placed under the control of the ministry of war. The length of studies will be three years.

Retirement of Prof. St. Cyr.—After thirty-five years' service as a teacher at the Lyons Veterinary School, Prof. St. Cyr is compelled to retire because of ill health. In acknowledgment of his value and of the good work he has done, the Government has conferred on him the title of Honorary Professor, while the teaching staff has bestowed on him a model on which is the effigy of Bourgelat, as a souvenir of their regard for him.—*Veterinary Journal.*

Dr. Holcombe, State Veterinarian of Kansas, assures the governor that he and the Sanitary Commission are doing all they can to rid the State of glanders. Before an animal can be killed by order of the Commission, the law requires the inspection of the suspected animals by the State Veterinarian, who reports the case to the Sanitary Commission. A day of hearing is then set, and the owner given notice, so that he may appear with such evidence as he may wish to present. If the commission find the animal diseased, as charged, he is then ordered killed.—*National Live Stock Journal.*